THE PSYCHOLOGICAL RESEARCHES OF JAMES McKEEN CATTELL

A REVIEW BY SOME OF HIS PUPILS

ARCHIVES OF PSYCHOLOGY

EDITED BY

R. S. WOODWORTH

No. 30, APRIL, 1914

COLUMBIA CONTRIBUTIONS TO PHILOSOPHY AND PSYCHOLOGY,
VOL. XXII, NO. 4

BF
21
A7
no.30

NEW YORK
THE SCIENCE PRESS

AGENTS G. E. STECHERT & CO.; London (2 Star Yard, Carey St., W. C.); Leipzig (Hospital St., 10); Paris (76, rue de Rennes).

Northeastern University
Math/Psychology
Library

THE PSYCHOLOGICAL RESEARCHES OF JAMES McKEEN CATTELL

A REVIEW BY SOME OF HIS PUPILS

ARCHIVES OF PSYCHOLOGY
EDITED BY
R. S. WOODWORTH

No. 30, APRIL, 1914

COLUMBIA CONTRIBUTIONS TO PHILOSOPHY AND PSYCHOLOGY,
VOL. XXII, NO. 4

NEW YORK
THE SCIENCE PRESS

AGENTS G. E. STECHERT & CO.; London (2 Star Yard, Carey St., W. C.); Leipzig (Hospital St., 10);
Paris (76, rue de Rennes).

Psych
BF
21
A7
no. 30

PRESS OF
THE NEW ERA PRINTING COMPANY
LANCASTER, PA.

6 Feb 19[illegible]

FOREWORD

The pupils of Professor James McKeen Cattell, wishing to recognize in some public way his completion of twenty-five years of service as professor of psychology, have decided to attempt a review, in brief compass, of his work in psychology, with some notice also of recent developments in several lines in which he has been a pioneer. The actual preparation of the volume was entrusted to a few men who had themselves followed the lines of work to be reviewed. As the papers were intended, in part, to be presented at a reunion, no complete digest was aimed at, yet it is hoped that this volume may bear witness, in a modest way, to the high esteem and sincere affection in which Professor Cattell is held by the many whom he has trained and guided in the study of psychology.

40896

CONTENTS

Professor Cattell's Work on Reaction Time 1

V. A. C. Henmon

Professor Cattell's Studies of Reading and Perception...... 34

Walter F. Dearborn

Professor Cattell's Relation to the Association Method...... 46

F. Lyman Wells

Professor Cattell's Psychophysical Contributions............ 60

R. S. Woodworth

Professor Cattell's Studies by the Method of Relative Position. 75

H. L. Hollingworth

Professor Cattell's Relation to the Study of Individual Differences 92

E. L. Thorndike

PROFESSOR CATTELL'S WORK ON REACTION TIME

The measurement of reaction time antedates the application of experimental methods in psychology, having been undertaken first by astronomers to determine the amount of the personal equation in transit observations and later by physiologists to determine the speed of nervous conduction. The astronomical period begins in 1822 with the publication by Bessel, astronomer at Königsberg, of the results of comparative tests, which established the fact of persistent individual differences in recording the times of stellar transits, and extends roughly to 1865. The physiological period begins in 1850 with the publication by Helmholtz of the first simple reaction time measurements to electric stimulation and may perhaps be said to extend to 1874, the date of publication of Wundt's "Grundzüge der physiologischen Psychologie." The psychological period dates from 1865 with the publication of Donders and de Jaager's experiments on the times of discrimination and choice. At successive stages in the psychological period the major interest has centered about (1) the time-relations of simple and complex mental processes and their variations with the quality, intensity and complexity of stimuli (1865–88), (2) the effect of direction of attention upon reaction times (1888–1905), and (3) the introspective analysis of the reaction (1905–1912). Present-day interest in reaction times is not limited to any particular field but ranges rather widely over all the phases of the problem.

Bessel's observations were confirmed by many astronomers and the effects of numerous variations in conditions upon the personal equation were systematically studied. Attempts were made to reduce the amount of the personal differences by the substitution of chronographic for the eye and ear method of registration (Repsold (1828), Arago (1843), Bond (1851)), to eliminate "the annoying human element" entirely by photographic registration (suggested by Faye, 1849), or finally to determine accurately its amount (it was at first thought to be constant) and thus to allow for it in observations. The measurements made, and after 1838 published in the records of many observatories, were of individual differences in times between observers and not of differences between the true time of transits and the observed time. In 1854 Prazmowski suggested an apparatus with artificial transits for the latter purpose and later Mitchel and Hartman in the same year (1858) published experiments by this method, apparently the first reaction time measurements recorded by astronomers. The variability in individual differences and in the differences between the true and the observed time due to the nature of the stimulus, its magnitude, rate and direction of motion, methods of registration and particularly divers psychological conditions, and hence the impossibility of eliminating or of determining with sufficient accuracy the personal equation led gradually to a subsidence of interest in the problem by astronomers. After the investigations by Hirsch and Plantamour (1862) and Wolf (1863–64) the chief contributions to the knowledge of reaction times came from physiology and experimental psychology. The numerous complication experiments on the inertia of attention and in recent years the investigation anew of the factors affecting the recording of transits by Alechsieff (1900) and by Gunther (1909) with the improved technique and in the light of the knowledge of reaction times accumulated in the intervening years are, however, an evidence of the continued interest in the original problem.

The investigations of the personal equation suggested to physiologists the possibility of measuring the speed of nervous conduction. In 1830 Nicolai sought an explanation for the personal equation in the different velocities of nervous processes. J. Müller, however, in 1844 maintained that it lay rather in the difficulty of keeping attention on two impressions simultaneously. In 1850, Helmholtz, after determining the speed of conduction in the motor nerve of the frog (27 meters per second), measured the physiological time in man elapsing between the stimulation by a weak electric stimulus and the execution of a movement, finding a total time ranging from 125σ to 200σ, the first reaction times published. Differences in reaction time resulting from stimulations at two points at varying distances from the brain indicated by this method a velocity of the nervous impulse of 60 meters per second. Numerous investigations followed with diverging results. Hirsch (1861) found a rapidity of 34 m. in sensory nerves on a basis of touch stimuli to the hand and the foot, Schelske (1868) 31 m., Kohlrausch (1864–5) 94 m., v. Wittich (1868) 34 m. to 44 m., de Jaager (1868) 26 m., Marey (1868) 30 m., and Richet (1882) 50 m. The rate of conduction in motor nerves, determined by the same method as with frog's nerve and hence with cerebral processes excluded, was found by Helmholtz and Baxt (1867, 1870) to be 33.9 meters per second, though wide variations (30–90 meters) were found with temperature and in different parts of the same nerve. v. Wittich (1868) by the reaction time method found a rate of 30.3 m. Place

(1870) repeated Baxt's experiments and found agreement only for longer distances. He also found differences in the different parts of the median nerve, 62 m. in the forearm and 17 to 124 m. in the upper arm. Du Bois Reymond (1900) has since shown that the differences in rate of conduction in different parts of a nerve are in all likelihood due to smallness of the number of cases and irregularities in technique. Hermann in reviewing the evidence placed the speed of conduction in motor nerves of man at 40 m. Donders and de Jaager (1865–68) showed the difficulties of the reaction method, that the reaction time is very essentially modified by the kind of stimulus, mode of reaction and degree of attention, and hence that the solution of the problem of the speed of nervous conduction by this method is well nigh hopeless. The assumption in using the method was that the reaction time when different parts of the body were stimulated would vary only by the time of conduction. When stimuli were applied to the hand and foot alone differences due to variation in cutaneous sensitivity would not necessarily be revealed. The lack of correspondence between the length of conduction and reaction time was shown by Marey, Bloch, Exner, and others with the result that interest shifted to the relation of reaction time to the place of stimulation in the researches of Hall and v. Kries (1879), v. Vintschgau (1880), Buccola (1883) and others. In view of the results of these experiments the physiologist's interest in the reaction time method subsided. Just as in the case of the personal equation the psychologist still retains his interest in this fundamental problem. Cattell and Dolley (1889–93), Kiesow (1903), Lyon and Eno (1912), and Poffenberger (1912) with improvement in technique and apparatus reattacked it, as will be pointed out below.

Astronomy and physiology thus set unsolved problems to experimental psychology. Donders and de Jaager (1865–68) were most active in studying the psychological factors affecting the simple reaction time and were the first to direct attention to the possibility of isolating and of measuring by the reaction method the time of complex mental processes, such as discrimination and choice. Two methods were used to isolate these processes. In the one (Donders' *b*-method) two stimuli were employed and reaction was made with the right hand if one stimulus appeared and with the left hand if the other appeared. In the other (Donders' *c*-method) two stimuli were presented and reaction was made if one of the two appeared and no reaction if the other appeared. Donders believed that in the *c*-method there was added to the simple reaction (Donders' *a*-method) the process of discrimination, and that the time of discrimination could therefore be determined by a simple process of subtraction, while in the *b*-method there was involved discrimination and choice and that the time of choice could be obtained by subtracting the times by the *c*-method from those by the *b*-method. v. Kries and Auerbach (1877) accepted this analysis and studied chiefly by the *c*-method the discrimination time of sense qualities, intensities and localization.

Experimental psychology took its place among the sciences formally with the publication of Wundt's "Grundzüge der physiologischen Psychologie" in 1874. In it the simple reaction received its first adequate psychological analysis and reaction time measurements occupy a prominent place. The physiologist's vague and indefinite characterization of the central or cerebral processes as a "transformation of a sensory impulse into a motor impulse" was analyzed into three psychophysical processes: (1) entrance into the field of consciousness, or perception, (2) entrance into the focus of attention, or apperception, and (3) excitation of motor processes in the central nervous system, or volition. This analysis has remained since 1874 with some modifications a constant feature of Wundt's psychology of the reaction. In its latest form (1911) the complete or sensory reaction is still held to include in clear succession the three processes, perception, apperception, and volitional excitation, especially apperception and volitional excitation. In the abbreviated or motor reaction two sub-forms are recognized, in one of which the apperception of the stimulus alone is eliminated or takes place after the motor impulse is discharged, and in the other the perception and motor excitation coincide temporally and the reaction is a complete cerebral reflex. Time measurements get a significance beyond that dreamt of by the astronomers and physiologists, with the possibility of separation within the simple and the compound reaction of the times of perception, apperception and volition and of these from the purely physiological processes of conduction to and from the cortex. Wundt states in the first paper in the first number of the *Philosophische Studien* that psychological time measurements are the most important field for experimental psychology. There are thus good historical reasons for the relatively great attention given to them in the Leipzig laboratory organized by Wundt in 1879. The early numbers of the *Philosophische Studien* are largely given over to reports of experiments on the relation of the stimulus to sensation (Weber-Fechner law) and to reaction time measurements, together with methodological discussions relating to them. That Wundt still regards reaction time studies as of fundamental importance is indicated by the researches carried on in his laboratory in recent years and reported in the *Psychologische Studien.* The noteworthy characteristic of the later researches is the shifting of emphasis from the time values to the systematic experimentally controlled introspection of the different forms of volitional action and the transitions from one to another. The objective time measurements while *unentbehrlich* are merely means of control for accurate introspection under varied conditions of reaction.

In addition to his detailed analysis of the factors involved in the simple reaction Wundt (1883) gave a systematic critique of methods of isolating the times of discrimination, simple and complex choice, and association. Donders' *c*-method is held to involve not only discrimination but also choice between movement and no movement and hence can not be used to determine the time of discrimination. He proposes for the isolation of discrimination (Wundt's *d*-method) that the procedure be the same as in the simple reaction but that the reaction be made only after the stimulus has been cognized or discriminated by the observer. The differences between the times so obtained and the simple reaction will give the time of cognition or discrimination. For momentary stimuli Baxt's method of determining the interval necessary between a given impression and a succeeding stronger impression to prevent the former from penetrating to the focus of consciousness was endorsed. The interval which just permits the apperception of the first impression is the time of discrimination. This latter method did not long survive the criticisms of v. Kries and Auerbach and of Cattell.

Wundt's conception of the processes that are involved in the reaction and of the methods for isolating the times of higher mental processes has dominated the entire history of reaction time research and largely determined its character and methods. Aside from the study by Kraepelin (1883) of the effect of drugs upon reaction time and that of Moldenhauer (1883) on simple reactions to olfactory stimuli, the first researches in the Leipzig laboratory were directed to the analysis of the times of discrimination, simple and complex choice, and association. Friedrich (1879–80) determined with three subjects the time of the simple reaction to a white surface, black, four colors, and one to six place numbers, discrimination and simple choice (*c*-method) for black and white, and discrimination and complex choice (*b*-method) for black and white. Tischer (1883) made a similar set of experiments using as stimuli sounds of five intensities. Merkel (1882–85) investigated the variation in choice times particularly when the number of movements is increased from one to ten. The stimuli were Arabic and

Roman numerals from one to five with reactions by the ten fingers. Trautscholdt (1883) made some rough experiments to determine the time of association. The experimenter called out a word and recorded the time by a hand movement whereupon the observer reacted by hand as soon as an associated word, which was later reported, came to consciousness. The average free association time for four observers was 727σ.

A comparative study of the results of these investigations together with those of Donders and de Jaager (1865–68), v. Kries and Auerbach (1877), Ott and Prendergast (1880), Tigerstedt and Bergquist (1883), and Buccola (1883) shows but little agreement on discrimination times by either the *d*- or *c*-methods. Even where the stimuli are approximately the same, *e. g.*, two colors, the agreement is slight. The times of discrimination for two colors by the *c*-method obtained by v. Kries and Auerbach were 12σ and 34σ, by Ott and Prendergast 50σ and 42σ, by Obersteiner 120σ, by Friedrich 152σ, 184σ, 183σ, and by Buccola 52σ. A tabulation of results in fairly comparable experiments by the other methods shows no greater agreement. Among the causes that lead to such divergences in results are the smallness of the number of cases on which the time values are based, variation in the amount of practise by different observers, variable errors in the control of the time recording apparatus and in mode of stimulation and reaction, and lack of system in the rejection of times that are regarded as non-typical. Donders and de Jaager's times of discrimination and choice for colors were based on 10, 29, 43, 41, and 24 reactions, respectively, for five observers. Friedrich's simple choice times are the result of 50, 24, and 51 reactions with three observers and the complex choice times of 30 reactions with one observer. The influence of practise and the variability in the recording apparatus upon the results is difficult of determination from the published reports, but it must have been considerable. Kraepelin (1880–3) first emphasized the importance of the greatest possible practise for reliable results and reported his experiences with the Hipp control apparatus and with various sources of electric current. Arbitrariness in the rejection of reactions regarded as non-typical makes any estimate of the validity of v. Kries and Auerbach's results difficult and doubtless explains the astounding correspondence in choice times obtained by Merkel, the times for nine observers being 24, 21, 21, 21, 23, 24, 23 and 21σ. These important sources of error were greatly reduced or eliminated entirely in the researches by Professor Cattell, which it is the main purpose of this paper to review.

Professor Cattell's first investigations, begun in America and completed in the Leipzig laboratory during the years 1883–1886, represent in point of number of experiments and in range of factors systematically studied the most notable and comprehensive contribution to reaction time measurements so far made. His experiments furnished new and valuable data on the inertia of sense organs, rapidity of conduction to and from cerebral centers, the influence of the quality and intensity of stimuli, mode of reaction, the significance of the time interval between the signal and the stimulus, the effect of varying degrees of concentration in the foreperiod, the effects of practise and fatigue, and an analysis of the times of cognition, discrimination, choice, association, and judgment. This enumeration makes it evident that Cattell's research ranges over practically the whole of this field of experimental work and that no other investigation of the time-relations of mental phenomena is comparable with it. The thoroughness with which these factors were studied is indicated by the fact that in his doctorate dissertation alone, which did not include the experiments on the time of recognizing and naming letters, words, pictures and colors, on the inertia of the eye and the visual center, and on association, more than 12,000 reactions on each of two subjects were obtained. A characteristic feature of the entire series of investigations is the attempt to minimize the influence so far as possible of involved methods, complicated apparatus, and the artificial conditions of the laboratory and to determine under conditions approximating those of daily life "the time it takes a man to perceive, to will, and to think." Cattell's major interest is neither in the detailed introspective analysis of the processes involved in cognition, discrimination, reproduction and judgment, nor in the subtleties of method of isolating these processes, but rather in the psychophysical determination of the time taken up in the mental operations in which we are constantly engaged. The possible value and application of the results of the experiments in education, diagnosis of disease, and practical life are suggested whenever they seem to have an important bearing. How far psychology as an experimental science has been advanced by the controversies since 1888 over the theories of the nature of the reaction, the sensory-motor distinction, and the introspective analysis of the reaction consciousness, is a matter upon which students of reaction time would perhaps not agree. Many at least would hold with Cattell that psychology can not rank as coordinate with the physical sciences without exact measurements and would question whether the introspective analyses of the reaction consciousness deserve the name. Introspective testimony unsupported by reliable objective and reproducible measurement, which characterizes much recent reaction-time research and is often given with great confidence, brings doubtful conviction. At any rate many lines of inquiry opened up or suggested as fruitful in possibility by Cattell have not been followed up as vigorously as could have been expected because of the shifting of interest to mental analysis and away from psychophysical measurements.

The first series of experiments published constituted a notable contribution to the psychology of reading and have become, as Erdmann and Dodge pointed out in 1898, the fundamental investigation in this field of work. Under simple but exact conditions approximating those of normal reading there were determined the time required to see letters and read them aloud or to count them, to read lists of isolated words or words in context in the native language and in foreign languages as rapidly as possible and at the normal rate of reading, and the time required to perceive and name colors and pictures of common objects. The second equally important contribution was an investigation of the sensitivity of the retina to colors, letters and words, the relative legibility of the letters of the alphabet, and the range or scope of consciousness for lines, numbers, letters, words and sentences. These well-known researches are discussed elsewhere in this volume and are mentioned here only as forming an important part of Cattell's investigation of time-relations of psychophysical processes.

Professor Cattell's "Psychometrische Untersuchungen" or The Time Taken up by Cerebral Operations contains various improvements in the apparatus and technique of the reaction experiment, a new theory of the nature of the reaction, and the most systematic attempt to isolate the times of higher mental processes. The apparatus devised consisted of the well-known and widely used gravity chronometer for the control of the chronoscope and the exposure of stimuli, a lip key and a sound key for vocal reactions. The control apparatus was an improvement over that provided by Hipp, the lip key over that employed by Helmholtz, and the sound key over that devised by Donders. These standard pieces of apparatus are in current use in substantially the form they were first constructed. In the treatment of results, instead of arbitrarily rejecting times that were regarded as abnormal Cattell substituted a systematic procedure of taking 13 or 26 reactions in a series and eliminating the 3 or 6 reactions which deviate most from the average. Premature reactions and those that were abnormally long because of lapses in attention were thus systematically eliminated. All false reactions were recorded. Because of the high degree of practise attained by the two observers (*B* and *C*) the differences between the averages from all of the reactions in a series and the corrected averages were slight. This method of treating results has recently been strongly advocated by Deuchler (1912).

Wundt's analysis of the processes involved in the simple reaction had been unquestioningly accepted by the investigators reported above from the Leipzig laboratory. Cattell's conception differs radically from it. Wundt's theory assumes that the larger share of the reaction time is taken up by the processes of apperception and volition. Cattell holds that "if these processes are present at all they are very rudimentary. When a subject has had no practise in making reactions a will time precedes the occurrence of the stimulus. That is, the subject by a voluntary effort, the time taken up by which could be determined, puts the lines of communication between the center for simple light sensations (in the optic thalami probably), and the center for the coordination of motions (in the corpora striata, perhaps, connected with the cerebellum), as well as the latter center, in a state of unstable equilibrium. When therefore a nervous impulse reaches the thalami, it causes brain changes in two directions: one impulse moves along to the cortex, and calls forth there a perception corresponding to the stimulus, while at the same time an impulse follows a line of small resistance to the center for the coordination of motions, and the proper nervous impulse, already prepared and waiting for the signal, is sent from the center to the muscle of the hand. When the reaction has often been made the entire cerebral process becomes automatic, the impulse of itself takes the well-traveled way to the motor center and releases the motor impulse." Cattell thus makes of the simple reaction after practise a subcortical reflex. The processes of perception and apperception, so far as they are involved, occur in the afterperiod of the reaction and the volitional process occurs in the foreperiod. This conception of the simple reaction Cattell has steadily maintained. James's analysis of the simple reaction is identical with it. Wundt adopted it after Lange's experiments for the motor or abbreviated reaction but still holds to his theory for the sensory or complete reaction. Erdmann and Dodge and an increasing number of recent investigators support the reflex theory.

The isolation of the physiological time of conduction to and from the cortex and the latent time in sense organs and muscles from the psychophysical or cerebral processes had been a fundamental problem among physiologists. The most probable rate of conduction in the sensory and motor nerves, in spite of discordance in results noted above, was 33 m. per second. The rate in the cord had been found by Burkhardt to be slower probably and the nervous impulse to be delayed in entering and leaving the cord and in passing through a ganglion. The latent time in the muscle was on a basis of Tigerstedt's experiments probably 5σ to 10σ. Various experiments had shown a latent time in sense organs, short in the mechanical senses but relatively long in the chemical senses. For light Cattell assumes the time to be 15–20σ for daylight reflected from a white surface. Of the total time for reaction to light, which Cattell found after practise to approximate 150σ, about half the time or 75σ is probably taken up by physiological processes and the other half by psychophysical or cerebral processes. Exner assuming 62 m. per second as the probable rate of conduction in sensory and motor nerves, 8σ for sensory and 11–12σ for motor conduction in the cord calculated the time of cerebral processes to be 83σ where the total reaction to touch was 134σ. The uncertainty of the data on the times of the purely physiological processes and the variability of the times of the psychophysical processes with attention has centered interest upon total reaction times and the factors affecting them rather than the isolation of the physiological from the psychophysical processes. Exner's and Cattell's are the only important systematic attempts at a probable determination of the "reduced reaction time."

The first experiments determined the simple reaction time to light, the stimulus being a white surface exposed by the gravity chronometer. The averages from 520 reactions for the two observers were 150σ for *B* and 146σ for *C*. The corrected averages were 1σ longer and the mean variations reduced from 13σ and 11σ to 8σ and 7σ, respectively. These times are considerable shorter than those previously reported by Hirsch (200σ), Hankel (224σ), Donders and de Jaager (184–226σ), v. Wittich (194σ), Wundt (222σ), v. Kries (193σ) and Auerbach (191σ). Exner gave 150σ as the reaction time to light and Buccola in three series found the times to be 168σ, 151σ and 172σ. How far these differences, and they obtain in other sense-departments as well, are due to variation in kind or conditions of stimulation, to direction of attention, to practise, or to individual differences in observers can not be satisfactorily determined even yet. Wundt unhesitatingly attributes the differences to the direction of attention, Cattell, Berger, Exner and Buccola using the motor reaction and the others the sensory. The fact that the mean variations of his own visual reactions is

20σ and those of Cattell 8σ to 10σ, and the fact that Alechsieff's six subjects with natural reactions varying from 174σ to 205σ when thoroughly practised in motor reactions each gave an approximately constant time of 150σ, are urged in support of his contention. This would seem to rule out individual differences but many observers find that they persist long after practise brings any essential change in the length of the reaction. Likewise many observers have found that after practise has rendered the reaction automatic the direction of attention to the stimulus or to the movement has no appreciable effect or at most a very slight effect on the length of the reaction.

The reaction times with the right hand and with the left hand were found to be the same, the times for the left hand being only 3σ longer for *B* and 1σ for *C*. This problem of the relation of symmetrical movements, executed simultaneously or in isolation, and the analysis of the reacting movement itself have been the objects of special study in recent years. Kiesow (1910) found a shorter reaction time with the right hand for four right-handed subjects (av. 3.4σ) and a shorter time with the left hand for three left-handed subjects (av. 5σ). Poffenberger (1912) found smaller differences when both eyes were stimulated, a left-handed observer giving shorter times with the right hand, but longer when one eye was stimulated, a right-handed subject giving faster reactions with the left hand. Sinn (1909) concluded that the left hand has greater capacity for executing automatic and reflex movements and the right for consciously motivated movements, with which Salow (1911) finds the results of Moore (1904) and Ach (1905) in agreement. In summarizing the evidence, Salow holds that the left hand in simple sensory reactions attains its maximal practise more quickly than the right hand and is always somewhat faster. In motor reactions and in choice reactions the differences are insignificant. When reacting simultaneously with both hands Féré (1889) found a lengthening of the reaction times and a preference for the right hand. Külpe (1891) studied the effect of direction of attention in simultaneous movements of the two hands. Smith's (1900) observations that some observers gave a preliminary antagonistic reaction, the lifting movement being preceded by a movement of the finger downward led in the studies of Judd, McAllister and Steele (1905) to a detailed analysis of the reacting movement itself.

The reaction time when the response was made with the speech organs was of especial interest for the experiments on reading. It was determined by two methods. In one the observer as quickly as possible after the appearance of the light said "Jetzt" and a second observer reacted to the sound. Deducting the simple reaction time to sound of the second observer gave the time of the speech reaction of the first observer. In the other method the lip key and the sound key were used. The speech reactions were about 30σ longer than the hand reactions and the results by both methods were in fairly close accord. How far this difference between the hand and vocal reaction is conditioned by practise and how far it is a relatively constant quantity can not be determined. The response to a white light by "Jetzt" is no less arbitrary than that of lifting the hand from a key. The associative connections vary in naturalness and facility and are modifiable by practise. This becomes an important consideration for instance in determining the time required to find the name of a word by subtracting the time required to distinguish a word without naming it. The strength of the associative connection in the responses in the two cases would differ by some amount to be eliminated by equivalent practise. Erdmann and Dodge's (1898) experiments show this, for with simple reactions to light the finger reaction gave for *D* 173σ and for *E* 178σ, reaction with the lip key for *D* 159σ and for *E* 167σ, and with the sound key gave for *D* 222σ and for *E* 187σ. The simple reaction to a letter by a finger movement was for *D* 196σ and for *E* 180σ, the reaction by naming of the letter was for *D* 227σ and for *E* 218σ with the lip key and for *D* 434σ and for *E* 383σ with the sound key. Both individual differences and the differences in the three modes of reaction are very striking. Erdmann and Dodge give the results, but do not attempt to account for them.

Cattell and Berger studied the effect of intensity of stimulus. Hankel, v. Wittich, Vintschgau and Hönigschmied, v. Kries and Auerbach, René, and Buccola had observed that the reaction time to electrical stimuli became shorter with increasing intensity, Exner had shown such variation for visual stimuli and Wundt for sound, but the problem had been incidentally rather than systematically studied. Experiments were made by Berger and Cattell with visual, auditory and electrical stimuli. The visual stimuli were of eight intensities arranged with reference to the light produced in a Geissler's tube by an induction current from six Daniell cells. Five weaker intensities were secured by putting smoked glass before the light, and two lights brighter by an undetermined amount than the normal by means of lenses. Four intensities for sound and electrical stimuli ranged from near the threshold to very high intensities with intermediate steps equal as far as could be judged. The results of experiments showed that the greater the intensity of the stimulus the shorter the reaction time but the differences were not such as to permit the formulation of a general law. The effect of intensity was most marked near the threshold and slight in the region of normal intensities. The number of cases was too small, however, to determine the differences with accuracy.

Cattell and Dolley (1893) recur to the problem of intensity in the study of reaction time and the velocity of the nerve impulse. They found, as before, the reaction time to electrical and tactile stimulation to decrease with increasing intensity. The decrease for three intensities of impact expressed in terms of the height of the fall of the hammer was slight. In reporting the results Cattell says: "The reaction time thus becomes shorter as the intensity of the stimulus is increased but the difference in time is small so long as the stimuli are moderately strong. The difference is, indeed, so small that it is obscured by the error of observation, but in the final average of the three thousand four hundred reactions the time was decreased 1.3σ when the intensity was increased from 15 to 30 grams, and decreased 1.7σ when the intensity was increased from 30 to 60 grams. If, as Fechner's law assumes, the intensity of the sensation increases as the logarithm of the stimulus, the reaction time tends to decrease inversely as the intensity of the sensation."

The dependence of reaction time upon intensity, area and duration of visual stimuli was taken up in the Columbia laboratory by Froeberg (1907) who formulated a general law that "in the middle of the scale the time of reaction tends to increase arithmetically as the magnitude of the stimulus decreases geometrically and that this applies to all three factors of the magnitude." On the influence of intensity of sound on the time of reaction the results are not in accord. While Berger and Cattell found a decrease with increasing intensity, Martius (1891) and Slattery (1892–93) contend that the intensity of the sound has no influence on the time of reaction, though the range of intensities studied was small. The experiments of Kiesow (1904) and Froeberg (1907) fail to support this contention. Dunlap and Wells (1910), however, still say: "That differences of intensity, as such, within the limits of what may be called satisfactory intensities, may cause differences of reaction time, is not established."

The relation of attention to the length of the reaction time was studied in relation to degrees of attention, the most favorable interval between the ready signal and the stimulus, and the effect of various modes of distraction. If the processes of apperception and volition are essential elements in the reaction, the effect of varying degrees of attention should be very marked. If, however, the reaction is an acquired reflex then the effect of concentration of attention would be slight. Three grades of attention, concentrated, normal and distracted, were studied with light, sound and electric stimuli. Four methods of distraction were employed—three beating metronomes, adding to any number 17 after 17 as rapidly as possible, variation in the interval between the signal and the stimulus from the normal interval from 3/4 to 1 3/4 up to 15 secs., or withholding of the stimulus entirely in half of a series of reactions. With the first method the reaction time to light was lengthened for B 2σ and for C 10σ, while that to sound was unaffected. With the second method the reaction times for electric light, electric shock, daylight and sound were lengthened for B 34σ and for C 22σ, while with highly concentrated attention without distraction the times were shorter than under normal attention for B 13σ but longer for C 3σ. The results show that the effect of varying degrees of attention on reaction time is slight and "support the hypothesis that a reaction is an automatic act, only needing the activities seated in the cortex to prepare the way."

With the third method, the much-studied problem of the optimal interval between the foresignal and the stimulus was opened up. Wundt had shown that reaction times without a warning signal were much longer than when a signal was given. Exner, v. Kries and Auerbach, and Cattell adopted an interval of approximately 1 sec. in the normal reactions. Cattell found that when this interval was lengthened to a maximum of 2 seconds the times for B were slightly shorter than the normal while for C the times were slightly longer, "concentrated attention not shortening his times and the delay interfering with the maximum of readiness." He concludes that attention can be held strained, that is the centers kept in a state of unstable equilibrium, for 1 second. When the interval was varied between the normal and fifteen seconds the times are considerably longer and the mean variations large. Owing to its importance in reaction time work the most favorable interval has been often studied. According to the review by Todd (1912), Estel (1885) found this interval to be 2 1/4 seconds, Mehner (1886), Wundt (1887), and Glass (1888) place it at 2 1/2 seconds, Lange (1888) at 2 seconds, Bertels (1889) at 2 3/8 seconds, Dwelshauvers (1891) of three intervals 1 1/2, 3 and 6 seconds found the shortest times with the 1 1/2 second interval, Bliss (1893) used a 2 1/2 second interval, della Valle (1907) found two favorable intervals one at two seconds and the other at six seconds and Breitwieser (1911) found the optimal interval to extend from two to four seconds. The rhythmic fluctuations with intervals from one to ten seconds obtained by della Valle were not shown in Breitwieser's results. The problem of degree of attention which had hitherto been regarded as of prime importance was overshadowed by that of direction of attention and what is attended to, and hence on it little has been done in recent years.

The major part of Professor Cattell's investigation concerns the central stages in the complex reaction, perception or discrimination time (*Unterscheidungszeit*) and will or choice time (*Wahlzeit*) to use the conventional terminology. Perception time is defined "as the interval between sensation and perception (or between indefinite and definite perception, apperception), that is, the time passing after the impression has reached consciousness before it is distinguished." Will time is the interval between perception and the initiation of the motor response. By Wundt's *d*-method of determining discrimination time satisfactory results could not be obtained. Cattell says: "I apparently either distinguished the impression and made the motion simultaneously, or if I tried to avoid this by waiting until I had formed a distinct impression before I began to make the motion, I added to the simple reaction, not only a perception, but also a volition." He used therefore Donders' *c*-method for perception or discrimination time and the *b*-method for will or choice time. The *c*-method he held with Wundt to give both a discrimination time and a choice time. The nature of the perception or discrimination could, however, be altered without altering the will time and the length of the discrimination time under various conditions be determined. The supposition that the will time remains approximately constant was based on experiments by Berger and Cattell in which it was shown that while the discrimination time varied with the intensity of the stimulus the will time was unaffected.

The simple reaction time to light had been found to be for *B* 150σ and for *C* 146σ. The reaction times with discrimination and choice for black and white were for *B* 211σ and for *C* 241σ. The discrimination and simple choice times by subtraction become then for *B* 61σ and for *C* 95σ. This total time is equally divided between discrimination and choice giving thus in round numbers a perception or discrimination time of 30σ for *B* and 50σ for *C*, with the will time the same. When reaction was made with the speech organs without naming the total times for discrimination and choice were for *B* 65σ and for *C* 100σ agreeing very closely with those for the hand reactions. In the same way the perception time for colors, letters, long and short words in the native and in a foreign language, and pictures of common objects was determined.

The will time or choice time was studied by allowing the nature of the reaction to vary with the impression, either by reacting with the right hand if one of two stimuli appeared and with the left if the other appeared, or by naming the stimulus. The first method was employed with pairs of colors and letters. The total times for discrimination and choice with colors were for *B* 145σ and *C* 190σ. These times compared with those on discrimination obtained by the *c*-method indicate that it took *B* 180σ and *C* 34σ longer to distinguish colors and to make the choice between two movements than between movement and quiescence. With letters the will time for choice between two movements is for *B* 11σ and for *C* 43σ longer than between making a movement and not making. Most of the experiments on will time were made by the second method of naming the stimulus where the response is natural and automatic. When letters, words, colors and pictures are presented in pairs the increased will or choice times required for naming are for *B* 15, 2, 24, 39σ and for *C* 37, 33, 101, 74σ, respectively. When the stimuli are not presented in pairs the process of choice becomes more complex, it being necessary to distinguish one object from a large number and name it. Thus the time needed to find the name of a letter was for *B* 143σ and for *C* 176σ, to name a short word *B* 104σ and *C* 114σ, to name a color *B* 286σ and *C* 400σ, and to name a picture of a common object *B* 251σ and *C* 278σ. This procedure is typical of the methods employed since the time of Donders and endorsed by Wundt. It is carried out on a far more elaborate scale and into more complex activities than in previous investigations.

A vigorous controversy has centered about three problems here involved: (1) the validity of the subtraction method of obtaining the times of discrimination and choice; (2) the adequacy of the characterization of the processes as discrimination and choice; and (3) the propriety of dividing the total time equally between the two when the *c*-method is employed to determine the discrimination time. Buccola and Tischer had expressed some doubts as to the legitimacy of obtaining the will time by subtracting the time of reaction with discrimination from the total time of reaction with discrimination and choice, urging that the conditions of discrimination and volition are different in the two cases. Cattell expresses similar doubts about subtracting the time of simple reaction from that with discrimination and choice; for he says that he assumes, "though not without hesitation, that the corresponding physiological processes take up the same time as in the simple reaction." Münsterberg (1889), Külpe (1893), Erdmann and Dodge (1898), Müller and Pilzecker (1900), and Ach (1905) have urged that the variability of the associative preparation in the foreperiod and the variation of the motor adjustments under different conditions make the times obtained by subtraction incomparable and almost valueless. Ziehen (1900), however, still employs elimination by subtraction, Watt (1904) justifies it under proper conditions and Wundt (1911) insists that while the times of cognition, discrimination and choice are not absolute time magnitudes the times obtained by subtraction are, first, an approximate measure of the complexity of the processes and the inhibitions met with, and, secondly, an important factor in the analysis of the processes themselves. "In both respects the so-called cognition, choice, association reactions, etc., are not so much independent time values as rather relative delay values of the individual psychic factors in the total process. . . . " This view of the significance of the absolute time values Cattell would probably indorse. The relative increase in reaction times with variation in complexity of conditions is what he set out to determine. Later critics of elimination by subtraction, such as Erdmann and Dodge (1898) and Ach (1905) have failed to see this and have attributed to him a conception of the value of the absolute times which he nowhere expresses. The important inferences may be drawn from the total times of reaction quite as well as from the 'reduced' perception or will times and hence the measurements possess permanent value whatever attitude one may take toward the subtraction method. Wundt (1911) thus quotes Cattell's total times for naming colors, pictures, letters and words as the best determinations we have, instead of the reduced times.

The second problem is the analysis of the processes called discrimination and choice. Erdmann and Dodge (1898) in reactions with naming of letters and twenty-six familiar words of four letters maintain that there is in such cases no genuine discrimination but rather a process of cognition. Introspective analysis shows usually no visual imagery of other letters or words with which the presentation may be compared or from which it may be discriminated. Moreover, there is no genuine choice in naming reactions. Habitual association with the appropriate reaction takes place as a rule without consciousness of meaning, feelings of innervation, or auditory imagery on which the choice must be based. Cattell was not concerned with the distinction between discrimination and cognition, using now one term and now another, or the more general term, perception, in the English version. Likewise he recognizes that there is in naming reactions no genuine choice for he says, "I use the term 'Will-time' for lack of a better; in Germany 'Wahlzeit' is used. The motion is in most cases simply the result of the perception, and 'Association-time' might be used, were it not already taken up. 'Motor-time' would perhaps best explain the process, but might cause confusion." Whether the process involved in perceiving that one word is presented and not another shall be called cognition or discrimination and whether the finding of the proper name for a word involves choice or volition in the usual acceptance of the terms are matters of less moment from his point of view than that it should require no longer time to perceive and name a word than to perceive and name a letter, that the time of distinguishing some letters should be longer than for others, or that the time of finding the names of colors or pictures of common objects should be far greater than the time for naming letters or words. The bearings of these facts on the psychology of reading, on the legibility of letters, and on the varying strengths of habitual associations is of more importance than the scheme of classification of the reactions. Out of the voluminous discussion has come, on the one hand, a somewhat clearer distinction between cognition and discriminative reactions and a more accurate analysis, on the other, of the choice reactions between movement and no movement, between two or more movements, and in naming. Thus Wundt distinguishes in genuine choice reactions between the apperceptive reproduction and the impulsive reproduction of a movement and both from automatic coordinations, to which naming reactions belong, and in which no genuine choice is involved. Ach has attempted to study experimentally genuine choice by allowing the observer to choose between two simultaneously presented stimuli the one to which a predetermined reaction shall be made.

The third problem of the propriety of dividing the time of discrimination and choice equally between the two processes has not been so much discussed, partly because in later investigations by Titchener and others the application of Wundt's *d*-method for determining the times of cognition and discrimination was successfully carried out and partly because it introduced a constant choice time of 30σ or 50σ. While Berger and Cattell had found the will-time in discriminating light intensities to be approximately constant, other observers noted that it varied with the difficulty of discrimination or cognition and with practise. For Cattell's purposes the exact determination of the times of discrimination and choice was not the object of inquiry, for he says: "We may suppose that the time of the centripetal and centrifugal processes through the brain is about the same, and that the time used in the cortex is about equally divided between the perception of the light and preparation of the motor impulse; at all events the whole time is so short that, if we divide it equally between the processes of perception and volition the error can not be great."

The more important of the many determinations of what seem to be the time it takes *B* and *C* to perceive and find the names of various objects appear in the following oft-quoted table:

	B	*C*
Reaction-time for light	150	150
Perception-time for light	30	50
Perception-time for a color	90	100
Perception-time for a picture	100	110
Perception-time for a letter	120	120
Perception-time for a short word	120	130
Will-time for colors	280	400
Will-time for pictures	250	280
Will-time for letters	140	170
Will-time for words	100	110

The concluding paragraph indicates the conception underlying the long series of experiments. "We have thus found the time it takes us to see and name the objects which we spend a great part of our life in seeing and naming. We have not dealt with artificial processes or things outside the circle of our natural interests. If in the course of evolution, as is probable, the molecular arrangement of the nervous system becomes more sensitive and delicately balanced we may suppose that the times taken up by our mental processes become shorter, and we live so much longer in the same number of years. It will therefore be of great interest to make experiments such as these on the lower races, as well as on persons of different age, sex, occupation, etc." It is a matter of regret that methodological controversies have prevented thoroughgoing experiments of the character here suggested. Even the observation that the perception of a word and the naming of it requires no longer time than for a letter, confirmed by Titchener's (1893) experiments on cognition time and by Erdmann and Dodge (1898), has not, so far so far as the writer is aware, been tested on children of various ages.

The effects of varying degrees of attention (normal, concentrated and distracted), and of fatigue on complex reaction times were found to be far less than is usually supposed. Just as in the simple reaction, the degree of attention had but an inconsiderable effect on the reaction times. The effect of fatigue is likewise slight. Even in the remarkable series in which 1950 simple and complex reactions were made in a single day, the observer reacting almost continuously from early in the morning until late into the night, the fatigue effects were not marked. Very striking is the result that the most automatic processes, the simple reaction and the naming of letters, were the most affected. The effects of practise on complex reactions were especially tested by repeating at the close of the investigation the experiments on perception time for colors, letters and words. Practise decreased the times by amounts ranging from 20σ to 54σ. The choice reactions showed similar effects. After an interval of three months during which no reactions were made *B*'s times became in general shorter, markedly so in the case of choice reactions, while *C*'s times were but slightly altered.

The last division of Professor Cattell's investigation was the determination of the time of completely and partially constrained associations and the time of judgment. Aside from Galton's (1879) pioneer work on the time of free association, and that of Trautscholdt (1889) chiefly on free associations, nothing in this field had been published, which, as Cattell remarks, was a "pity, as experimental psychology seems to have its most hopeful outlook in this direction." The experiments on completely constrained association consisted in determining the time required to find the name of a picture in a foreign language, the time of translating words from the mother tongue into a foreign, the time of making the association between a well-known city and the country in which it is located, between a month and its season, a month and the following or the preceding month, an author and the language in which he wrote, a great man and his vocation, and the time of addition and multiplication. The average time for these associations was 420σ for *B* and 436σ for *C* with wide variations between the two observers due to the familiarity of the associations, temperament and calling.

The experiments on partially constrained associations determined the time of associating to a country some city located in it, to a season some month, to a language some author, to an author some work, to a noun representing a class of objects some example, to a picture of an object some part of it, to a concrete noun some part of the object, to a picture or a name of an object what it is used for or what it does, to a substantive an appropriate adjective, and to a verb an appropriate subject or object. The times in such ambiguous associations with a larger liberty in response were longer and very variable.

The last group of experiments determined the time of judging lengths of lines, the number of lines in a group varying from 4 to 15, and the relative greatness of eminent men.

Later three series of experiments (1885–88) carried on at Leipzig and in England furnished data on the time of free associations and a statistical study of their nature. The first gave under exact conditions the times for free association with concrete nouns, abstract nouns and verbs. The second determined the average times for single associations to words in lists of ten, the observer seeing the words in order and giving as quickly as possible what each suggested. In the third the observers wrote the associations suggested in 20 secs. by a stimulus word. The second series given to university graduates, American college students and to students in German gymnasia showed variations in time of association from one to seven seconds and wide personal differences. More striking were the results with the third series given in London and Dublin schools and to college and university students which show a marked shortening of the time of association with mental growth and education and in the case of the students of a London school a positive correlation between association time and class rank.

The suggestion that in such measurements of association times experimental psychology would have its most hopeful outlook has been amply realized. Controversies over elimination by subtraction, the analysis of the processes involved, and the influence of the instructions and attitude in the foreperiod have not prevented a wide application of time measurements to a study of a variety of factors that determine the speed of association. That the time would vary with the familiarity of an association would require no experimental confirmation. The exact measurement, however, reveals variations in the strength of associative connections that would escape unaided observation. Cattell's results that it should require almost twice as long to multiply one-place numbers as to add them, that to make the progressive association from a month to the following month or from a letter to the following letter only half as long as the regressive association to the preceding month or letter, that it should take less time to make an association from part to whole than from whole to part, or from a special class to a more general than from the more general to the special, that the interference of associations where the response is ambiguous should lengthen the time of association, and that there should be such wide personal differences and differences due to age and maturity, suggest the value of the associative reaction in discovering the strength of associative connections that would not otherwise be more than vaguely suspected.

The association reaction has been developed and applied far beyond cognitive, discriminative and choice reactions. Two tendencies have been at work, in one of which the emphasis has been laid upon its value in the analysis of memory, forms of association, and thought processes (Müller and Pilzecker (1900), Watt (1905), Messer (1906), etc.) and in the other the emphasis has been upon its value for individual psychology. The influences of age, practise, sex, fatigue, drugs and pathological conditions have been investigated in important researches by Kraepelin (1893), Aschaffenburg (1896–1897, 1904), Ziehen (1898, 1900), Jung (1906), Wreschner (1907), etc. The value of the associative reaction in disclosing associative connections which an observer wishes to conceal, in uncovering emotional complexes that produce mental disturbances, and in discovering the early unsuspected influences on the mental life of the child, has already found a wide extension in the applied psychology of jurisprudence, medicine and education.

Upon the completion of the investigations summarized above, Professor Cattell recurs to the problem of the speed of nervous conduction. His research with Dolley "On Reaction Times and Velocity of the Nervous Impulse" is the most important investigation in this field by the reaction time method. The research was begun at the University of Pennsylvania in 1889 and completed at Columbia University in 1893. The number of reactions in the first table alone is greater than in all the many investigations of this problem previously published. This fact together with the improvements in the Hipp chronoscope and in the mode of stimulation and reaction justifies the claim that "these experiments supersede those hitherto made" on the velocity of the nervous impulse.

Numerous improvements were made in the construction and regulation of the chronoscope, notably the rewinding of the electromagnets with coarser wire to reduce the latent time of magnetization and demagnetization, and in the gravity chronometer for control of the chronoscope and exposure of stimuli, especially the substitution of wheel contacts for making and breaking a current instead of the mercury contacts nearly always used hitherto. With the wheel contacts it is possible among other things to vary conveniently and accurately the interval in the case of electric stimulation. If electric shocks are applied at varying distances from the brain, the shock at the more remote point would have to be applied earlier than at the nearer point to be felt simultaneously with it and this interval would presumably be a measure of the time it takes for the impulse to travel the distance between the two points. Experiments of this sort were begun but apparently not completed. Bloch (1875) had made similar experiments on the interval necessary for the fusion of sensations which led him to conclude that the rate of conduction in sensory nerves was 192 meters per second and in the spinal cord 194 meters per second. Lyon and Eno (1912) have developed the method with interesting results. The rate of nervous impulse which is indicated by their preliminary experiments, viz., 10 meters per second, accords, however, neither with the results of reaction time measurements nor with other experiments on the rate of the nervous impulse.

The first experiments were made with electrical stimuli applied to four points of the skin, two on the arm over the median nerve 30 cm. apart and two on the leg over the posterior tibial nerve 50 cm. apart. With the variation in point of application, if the other factors in the reaction time remain the same, the difference in the time of reaction should be a measure of the speed of the sensory impulse in the nerve and spinal cord. With a variation in the mode of reaction, the other factors remaining constant, the speed of the motor impulse would be determined. However, a fundamental difficulty arose in that the same physical stimulus at the four points produced sensations varying in quality and intensity and reactions varying in facility. This difficulty led ultimately to the substitution of touch for electric shock as stimuli and the results of the experiments on touch are regarded as more satisfactory. Many interesting psychological observations are reported of the sensory effects of electrical stimulation of the skin and their variation with the part of the body stimulated, application of the positive or negative pole, frequency of stimulation, and the size of the electrode. The excess in the time of reaction when the stimulus was applied to the two points on the arm was for *D* 14.2σ and *C* 6.1σ, on the leg 22.3σ for *D* and 5σ for *C*, indicating, on the assumption that the difference in time is due to the difference in the length of the nerve travelled, a velocity of the sensory impulse in the median nerve of 21.1 meters per second for *D* and 49.5 meters for *C* and in the posterior tibial nerve of 31.1 meters per second for *D* and 64.9 meters per second for *C*. Cattell and Dolley are however not prepared to accept these velocities as valid. The differences in the times of reaction are valid but the variation in the two observers and in the same observer at different times suggest that the differences are not due to the velocity of the impulse but must be attributed to cerebral processes.

When the electric shock was applied to the upper leg in one case and the upper arm in the other the differences in reaction times with the hand were for *D* 25σ and *C* 26.6σ, and when applied to the lower leg and lower arm for *D* 26.9σ and for *C* 28.1σ. When the reaction is made with the foot and the stimulus applied to the lower leg and lower arm the differences in times are for *D* 18.9σ

and for *C* 18.7σ. The time of reaction is thus about 26σ longer with reaction by the hand and about 18σ with reaction by the foot when the impulse must traverse the spinal cord and from the lumbar to the brachial plexus, which indicates a velocity of about 15 meters per second in the sensory tracts of the spinal cord.

To find the velocity of the motor impulse the differences in times of reaction with the hand and with the foot were determined. These differences when the stimulus was applied to the lower arm were for *D* 37.7σ and for *C* 54.4σ and when the stimulus was applied to the lower leg for *D* 29.7σ and for *C* 45σ. These differences may partly be due to the fact that the hand reaction is more automatic or it may be due partly to the delay in traversing the motor tracts from the brachial plexus. If the differences were solely due to this delay the velocity of transmission in the motor tracts of the cord would be about 10 meters per second.

The difficulties encountered in determining the velocity of the sensory impulse owing to the varying effects in quality and intensity of the same objective stimulus led to a large number of reactions (10,400) to study these factors. When two points at varying distances from the brain but giving approximately similar sensations were stimulated the reaction times were almost exactly the same. When the same stimulus was applied to two points nearly equidistant from the brain but giving different sensations, the reaction time was 10.2σ shorter for the point for which the sensation was more intense, when the stimuli were varied in intensity and area the reaction times increased as the stimuli decreased, when the stimuli were applied to the hand by which the reacting movement was made the times were shorter, and when the reacting movement is made from the shoulder the time is longer than when made from the forearm, wrist or finger, the latter times being nearly the same. All of these facts indicate that satisfactory measurements of the velocity of the sensory impulse can be secured only if the physiological intensities of stimulation are made equal. This was not attempted because it seemed impossible to allow for differences in reaction due to the place of stimulation and because better results could be secured with mechanical touches or blows than with electric stimulation.

Touch stimulation gave more satisfactory results because "the same objective force of blow is followed by the same subjective sensation more nearly than in the case of electric stimulation," because the same point can be stimulated time after time and day after day, and because consequently the variable error in the reactions is much smaller. Various methods were employed for applying the blow, the best of which was a hammer with weights of 15, 30, and 60 grams screwed on the tip of a light aluminium tube falling a perpendicular distance of 20 cm. The current was closed at the instant the blow was given. With this instrument reaction times to blows on ten parts of the body were obtained, the weight being 30 grams and reactions made both with the hand and with foot. The times were about 10σ shorter for touch than for electric stimulation. The reaction times for points on the upper and lower arm and for the upper and lower thigh are practically the same in all cases, the time of transmission from the lower to the upper points being almost exactly counterbalanced by a more facile cerebral reflex with the lower point. Hence the conclusion is reached that velocity of the sensory impulse can not be determined by the reaction method. The difference in times when the stimulus was applied to the arm and when applied to the thigh was 7.8σ and if this difference is due to the transmission for the lumbar to the brachial plexus of the cord, the velocity of the nervous impulse in the sensory tracts would be 40 meters per second.

The general conclusion of the research for which in completeness and thoroughness there is no counterpart in reaction-time literature and few in the whole field of experimental psychology, is that the velocity of the nervous impulse cannot be measured by differences in reaction time. Nevertheless the experiments indicate that the velocity must be greater than that commonly accepted, viz., 30 meters per second.

Lange's discovery (1888) that the reaction time differed with the direction of attention, being approximately 100σ longer when attention in the foreperiod was directed to the stimulus than when directed to the movement to be made became an important incentive to the investigation of central or psychological factors. While we should not, however, all agree with Ach (1905) that Lange's distinction of sensory and motor reactions contributed more to their study than all preceding investigations put together nor that all previous researches possess scarcely more than a historical interest, it is nevertheless true that for more than a decade practically all reaction time research centered about this problem. Lange's results were confirmed by Wundt, Frickes (1889), Münsterberg (1889), Dwelshauvers (1891), Martius (1891), and Titchener (1892). Münsterberg found that while the distinction was valid the differences in the times was only 42σ for auditory reactions. Martius likewise found the distinction valid but with practised observers the differences were still less, ranging from 5σ to 20σ, while with unpractised observers the difference was approximately 100σ.

It is interesting to note that Martius anticipated the later analyses by Ach and others in attributing the small differences to the dominance of the instruction to react as quickly as possible. Titchener (1892) found with three observers an excess in time of sensory over motor reactions of 81σ, 84σ, and 97σ. Still other investigators, Cattell (1892), Dessoir (1892), Jastrow (1892), Bliss (1893), Hill and Watanabe (1893), Baldwin (1895), Flournoy (1896), Farrand (1897), and Angell and Moore (1896) found the distinction either not universally valid or a matter of individual differences in type of reaction. Cattell (1892) reported his own reactions to sound and electrical stimuli to be entirely unaffected by the direction of the attention to the stimuli or to the response. To determine whether the great amount of practise which he had had in reacting was the cause, he obtained results with another observer who knew nothing of Lange's observations and was without practise in reaction. The difference in sensory and motor reactions was only 3σ. A later series on the same observer after attention was called to Lange's results gave likewise negative results. With a third observer who had made approximately 2,000 reactions the sensory reaction to electrical stimuli was 80σ shorter than the motor reaction. Jastrow (1892) found the differences with the two reaction forms in one subject to be reduced to 4σ. Bliss (1893) noted that when attention was directed to the movement the times were usually longer rather than shorter, though in some cases a considerable shortening of the times resulted. Dessoir (1892) with eight observers found no differences in sensory and motor reactions and hence concluded that where differences had been obtained it must be due to the apparatus employed. Hill and Watanabe (1893) showed this conclusion to be erroneous and confirmed the results from the Leipzig laboratory.

Baldwin (1895) in two subjects found the sensory reactions to auditory stimuli longer than the motor by 18σ and 48σ, respectively, while in two other subjects the sensory times were shorter by 40σ and 25σ. The motor form of reaction was further analyzed into a kinesthetic-motor form in which with the eyes blindfolded or the reactions made in a dark room attention was directed to the movement of the hand and a visual-motor form where the attention is directed to the movement of the hand as seen. In one subject the visual-motor form is longer by 26σ, in another shorter by 21σ, and in a third longer by 3σ. Baldwin developed the type theory of reaction and became its chief protagonist. He sought to bring the reaction types into relation to verbal image types and to refer both to the same source.

Flournoy (1896) among his seventy subjects found those whose sensory reactions were the shorter, others whose motor reactions were shorter, and still others whose reactions were unaffected by the direction of attention. He divided his subjects on a basis of their shortest reactions into four groups or types: motor, central, indifferent and sensory. The motor type showed two subforms, a natural motor type and a constrained motor type. The central type included those who in a series attended now to the stimulus and now to the movement and those who divide attention with varying predominance to both stimulus and movement. The indifferent type included those who because of practise showed no differences in reaction times with the sensory or the motor adjustments. The sensory type with shorter sensory than motor reactions include two sub-types, kinesthetic-motor and visual-motor, in the former attention being directed to motor sensations or images and in the latter to the visual image of the reacting movement. Farrand (1897) published reaction times obtained from the pianists Rosenthal and Sievekind. The natural reactions of the former were distinctly sensory (118σ), the observer stating that his attention was always to the sound and never to the movement. His motor reaction times averaged 254σ. The latter stated that his attention was always directed to the hand and declared it impossible to react to the signal. His motor times averaged 117σ. In the discussion of these results Baldwin interpreted them as supporting the type theory. Cattell argued, however, that they merely indicate the inhibiting influence of attention to habitual action, that people react in the way they are accustomed to react and when this reflex character is disturbed the times become longer.

Angell and Moore (1896) found it possible to reconcile the contentions of the general and the type theory, by reference to the interrelations of attention and habit. They set out from the general conception that different habitual forms of attention as a result of inheritance and education would be expected. One of the three subjects gave shorter times for the motor reactions (10σ to 30σ), another shorter times for the sensory reactions (5σ to 25σ), and the third gave generally shorter times for the motor reactions. The voluntary adoption of a new form of reaction produced at first a period of confusion and wide time differences followed by a subsequent reduction and approximation of time values of the two forms with a final possible shorter time for the motor form. The results indicated in support of the type theory that "the origin of types is to be found in education, which must necessarily apply to single functions," that the sensory time may be shorter than the motor even after the latter has clearly emerged in consciousness, and that practise tends to decrease the differences. In support of the general theory they found that when both forms have reached a high degree of reflexness, the motor form is probably somewhat faster though not nearly as much as the early Leipzig results indicated.

The divergence in results led to widely varying interpretation of the nature of the simple reaction and the causes of the differences. Are the two forms of reaction fundamentally different or alike in kind? Is one a reflex and the other a voluntary action, is neither a reflex or are both reflexes? Lange maintained that the two forms were essentially different in nature. He adopted Cattell's conception of the reaction for the motor form holding that it is an acquired brain reflex while the sensory reaction is subordinated to volition and involves the processes of perception, apperception and volition as demanded in Wundt's analysis. Wundt's interpretation was similar. In support of the conception of the motor reaction as a reflex he points to two objective criteria, false reactions and premature reactions, which occur with motor adjustments but never with the sensory adjustments. Wundt later qualified his interpretation because of studies on the genetic development of the various reaction forms. The researches of Alechsieff (1900) and Bergemann (1906) were directed to this problem. Alechsieff began with a study of the natural or central reaction to determine whether it was a constant form alongside of the other two fundamental forms or whether it tended to develop into one or the other. He found the natural reaction times after some practice with six observers to range from 174σ to 205σ. Practise with the motor form led to an approximately constant time of 154σ for all observers. Subsequent practise in sensory reactions was exceedingly difficult and gave satisfactory results only with those observers whose natural reactions inclined to the sensory form. The sensory reactions showed two modes one at 240σ and the other at 205σ. Bergemann (1906) traced the effect of practise on the three forms of reaction to auditory stimuli. The practise experiments were continued until the times and the distribution curves became regular. The practise effects were most marked with the sensory reactions and least with motor reactions. The resulting motor times averaged 100σ and the sensory 120σ. These results show that practise with different modes of adjustment of attention acts unequally, that the transition from a natural or central reaction to a motor reaction is easier than to the sensory reaction, and that the terminus of the sensory reaction after practise is the motor reaction or the reflex. They are held by Wundt to furnish experimental confirmation of the development of voluntary into impulsive and finally into reflex action.

The conception of the sensory and motor reactions as fundamentally different in kind, the one a reflex and the other a voluntary action, was vigorously opposed by Münsterberg and Martius though on different grounds. If the motor reaction is a reflex then the carrying over of the distinction to compound reactions was improbable. However, Münsterberg (1889) found that the distinction did hold here as in the simple reaction whence he concluded not that the simple reaction is not a reflex but that choice reactions are also acquired reflexes. Martius (1891) repeated Münsterberg's experiments and failed to confirm his results. Martius argued that with a plurality of possible stimuli and responses a genuine motor reaction is impossible. With a motor adjustment to the five fingers of the hand, from the very ambiguity in the response the motor reaction should be longer rather than shorter and this he found generally to be the case.

Martius, the keenest opponent of the reflex theory, argued that since the motor reaction is shown by observers without practise it can not depend upon practise nor be fundamentally different in kind from the sensory reaction, that false reactions and premature reactions are not satisfactory evidences of the reflex character of the motor reaction since they occur frequently in sensory reactions, and that introspective evidence of the simultaneity of perception and response holds both for the sensory and motor reactions. The processes of perception and apperception are differences in degree of clearness, and since perception is necessary to the motor reaction it cannot be a reflex nor essentially different in kind from the sensory reaction.

Cattell (1892) held not that one form was an acquired brain reflex and the other a voluntary action, nor that neither were reflexes, but that with maximal practise both are reflexes and fundamentally alike in kind. He pointed out that in the habitual movements in daily life, such as piano playing, speaking, writing, reading aloud, play and manual work, attention is always directed to the end and never to the movement. Whenever attention is directed to the movement to be made the coordinations are disturbed and become less automatic. Anything then that makes the reacting movement less reflex, be it attention to the movement or to the impression, would lengthen the reaction time. When therefore the attention is directed to the sense impression the time may be lengthened because the motor impulse is not kept in readiness to discharge. From this point of view the length of reaction time is not a function of the direction of attention, nor is it a matter of *Anlage* as the type theory maintains, but essentially a matter of practise and habituation. Many observations in recent years go to support Cattell's theory of the reaction, notably the frequent observation that the motor adjustment of attention is more difficult than the sensory (Sinn and others), that false reactions occur with almost equal frequency in both, and that the variability of the motor reactions is greater than that of sensory reactions.

While the type theory still survives in some quarters its untenability is now generally recognized. The supporters of the general theory concede that as a result of temperament or training an observer's natural reactions may show a predisposition or preference for either the sensory or motor form. They maintain on the other hand that the sensory or motor adjustments may be voluntarily adopted and developed by training. The controversy may be disposed of by adopting Deuchler's (1911) distinction between form of reaction and type of reaction. When maximal practise in the two adjustments is attained the differences in reaction times, as shown in the recent work of Bergemann (1906), Sinn (1909), and Breitwieser (1911), either are very small or disappear altogether. Whether the sensory reaction terminates after long continued practise in the motor reaction, or whether the automatization of each takes place independently of the other with the ultimate disappearance of time differences, is perhaps still a moot question. The latter seems in the present state of our knowledge the more likely hypothesis.

The sensory-motor distinction has lost much of its significance as a result of the introspective analyses of the reaction especially by Ach (1905), Wirth (1908) and Deuchler (1911). Ach pointed out that the attention to the stimulus or to the movement is not the essential factor in determining the differences in reaction-times but rather the observer's attitude toward the intention to react in its relation to the stimulus or to the movement. While there are two chief directions which attention may take, each is capable of further subdivision and requires more exact definition. This has resulted in analyses of the various attitudes reagents assume in the foreperiod of simple and complex reactions, the contents of consciousness in these attitudes, the effect of different forms of instruction, in all of which the time values are secondary or negligible factors.

Theoretically it would seem that the number of sub-forms is dependent upon the number of just noticeable differences in degrees of attention to the stimulus and to the movement. In the sensory reaction maximal attention is directed to the stimulus accompanied by the intention to react. The intention to react in the foreperiod may range in consciousness from zero to any degree less than that of attention to the stimulus. Flournoy (1896) found two sub-types, Deuchler (1911) three sub-forms and Ach (1905) five such different and distinguishable attitudes in the sensory reaction. The number, however, seems to depend not upon the number of distinguishable degrees of attention but upon the number of verbal distinctions that can be devised. In the motor reaction, while maximal attention is directed to the movement to be made, varying amounts of attention will be given to the stimulus. Here also the number of distinguishable degrees of attention to the stimulus should determine the number of sub-forms of motor reactions. Flournoy found two sub-types, Deuchler two sub-forms, and Ach four. Between the two extremes of attention to the stimulus with no attention or merely a just noticeable attention to the movement and of attention to the movement with no attention or merely a just noticeable attention to the stimulus lie not four or five or nine attitudes but an indefinitely large number. Just as we have devised names for a relatively small number of colors while we are able to distinguish about 150 color differences, so in the direction of attention we have now nine attitudes characterized and named while the number of attitudes possible of discrimination is yet undetermined. From this point of view both simple and complex reactions should be capable of an exact gradation which would be reflected in the reaction times. It is difficult to see why some of the present-day experimenters on the reaction consciousness go to the trouble of recording the times at all. They regard them apparently as non-significant and merely a means of securing accurate introspection.

Professor Cattell's most recent contribution (1902) on the time of perception as a measure of differences in intensity established the validity of a new psychophysical method, differences in sensations being measured, not by just noticeable differences, but by the time it takes to perceive them. Differences in sensations should be equal if it takes equal times to perceive them, while, if the differences are unequal, the greater the difference the shorter the time of perception. By this method it should be possible to arrange in accurate series, groups of differences in quality or intensity in every department of experience, simultaneously or successively perceived. Preliminary experiments on the time of estimating lengths of lines were begun at Leipzig (1888) and the problem is recurred to in 1893 and again in 1899. The publication of results by the method bore on the measurement of differences in the intensity of light in which it was shown that the time of perception increased uniformly as the differences in intensity decreased. The number of experiments was not sufficiently large to determine the exact nature of the increase nor to correlate the results with those by other psychophysical methods. Münsterberg (1894) applied the method to the measurement of linear magnitudes and reached the conclusion that the psychophysical law holds approximately. Henmon (1906) applied it to the measurement of differences in sensations of color, linear magnitudes, and pitch, together with an application to testing for color-blindness, Nakashima (1908) to the perception of lifted weights and Bourdon (1910) to more complex processes of identification, resemblance and localization. Related experiments on the time of sensory judgments in relation to their accuracy have been reported by Martin and Müller (1899), Angell (1901) and Henmon (1911). The fact shown in all these experiments that the discrimination reaction time varies uniformly with the differences to be distinguished suggests the possibility of a wide application of the method in individual psychology comparable with that of the association reaction already accomplished.

This review serves to indicate that the two most prominent contributors to the literature of reaction time have been Professors Wundt and Cattell. The former has been the chief protagonist of the apperception theory of the simple reaction, the latter of the reflex theory. The former at least in recent years has emphasized the qualitative aspect of the reaction, the latter the quantitative. To the former the measurement of reaction time is of value in securing psychological results through introspection under exact and controllable conditions, to the latter its ultimate value lies in the determination of psychophysical constants. What future developments in time measurements may bring forth "lies on the knees of the gods." While, as Titchener says, "there can be but little doubt that future psychophysical investigations will play into the hands of psychology" there is likewise little doubt that important results of subjective observation will have the sanction of exact psychophysical measurement.

V. A. C. Henmon

BIBLIOGRAPHY

1. Über die Zeit der Erkennung und Benennung von Schriftzeichen, Bilder und Farben, *Philos. Studien*, II., 4, 1885, pp. 635–650.
2. Über die Trägheit der Netzhaut und des Sehcentrums, *Philos. Studien*, III., 1, 1885, pp. 94–127. Published in Abstract—The Inertia of the Eye and Brain, *Brain*, Vol. XXXI., 1885.

3. Psychometrische Untersuchungen, *Philos. Studien*, III., 1, 1885, pp. 1–72. Reprinted as a doctorate dissertation, 1886. Published as The Time Taken Up by Cerebral Operations, *Mind*, Vol. XI., Nos. 42, 43, 44, 1886, pp. 220–242, 377–392, 524–528.
4. The Influence of the Intensity of the Stimulus on the Length of the Reaction Time, *Brain*, Vol. XXXII., 1886, pp. 512–516.
5. Psychometrische Untersuchungen. Dritte Abteilung. VI. Die Association unter willkürlich begrenzten Bedingungen, *Philos. Studien*, III., 3, 1885, pp. 241–250. Published as Experiments on the Association of Ideas, *Mind*, Vol. XII., No. 45, pp. 68–74.
6. Mental Association Investigated by Experiment (with Sophie Bryant), *Mind*, Vol. XIV., No. 54, 1888, pp. 230–250.
7. Aufmerksamkeit und Reaction, *Philos. Studien*, VIII., 3, 1892, pp. 403–406.
8. On Reaction Times and the Velocity of the Nervous Impulse (with C. S. Dolley), *Memoirs Nat. Acad. of Sciences*, Vol. VII., 2, 1893, pp. 393–415. Published in Abstract, *Psychol. Review.*, Vol. I, No. 2, 1894, pp. 159–168.
9. Chronoscop und Chronograph. *Philos. Studien*, IX., 2, 1893, pp. 307–310.
10. The Time of Perception as a Measure of Differences in Intensity, *Philos. Studien*, XIX., 1902, pp. 63–68.

PROFESSOR CATTELL'S STUDIES OF PERCEPTION AND READING

One of the chief problems which engaged the attention of Wundt and his pupils in the early days of the newly established laboratory for experimental psychology at Leipzig concerned the measuring of the rapidity of the mental processes. To the group of studies upon this problem belongs that of Professor Cattell, "Ueber die Zeit der Erkennung und Benennung von Schriftzeichen, Bilden und Farben,"[1] published in 1885 in the second volume of the *Philosophische Studien* and republished in condensed form in *Mind* under the title, "The Time it Takes to See and Name Objects."

The methods of the experiment, as the author notes, were intentionally simple in order to do away with involved methods and complicated apparatus, which had handicapped some of the earlier experiments in this field, and to approximate as closely as possible the condition of actual life. Letters were posted on a revolving drum and the rate was determined at which they could be read aloud as they passed by a slit in a screen. The following is a summary of the findings: "It was found that the time varied with the width of the slit. When the slit was 1 cm. wide (the letters being 1 cm. apart) one letter was always in view; as the first disappeared the second took its place, etc. In this case it took the nine persons experimented on (university teachers and students) from 1/3 to 1/5 sec. to read each letter. This does not however give the entire time needed to see and name a single letter, for the subject was finding the name of the letter just gone by at the same time that he was seeing the letter then in view. As the slit in the screen is made smaller the processes of perceiving and choosing can not so well take place simultaneously, and the times become longer; when the slit is 1 mm. wide the time is 1/2 sec., which other experiments I have made prove to be about the time it takes to see and name a single letter. When the slit on the contrary is taken wider than 1 cm., and two or more letters are always in view, not only do the processes of seeing and naming overlap but while the subject is seeing one letter, he begins to see the ones next following, and so can read them more quickly. Of the nine persons experimented on four could read the letters faster when five were in view at once, but were not helped by the sixth letter; three were not helped by a fifth and two not by a fourth letter. This shows that while one idea is in the center, two, three or four additional ideas may be in the background of consciousness. The second letter in view shortens the time about 1/40, the third 1/60, the fourth 1/100, the fifth 1/200 sec."[2]

---

[1] *Phil. Studien*, II., s. 641.

These results may also be considered as simple determinations of the limits of consciousness, that is, of the number of objects which can be considered by consciousness at one time. They were later confirmed by Professor Cattell in an extended series of tachistoscopic experiments concerned directly with this problem. The latter determinations were made on eight subjects and showed that "on an average consciousness can at one time grasp four numbers, three to four letters, two words, or a sentence composed of four words. The letters are slightly more difficult to grasp than the numbers, every combination of numbers making a number that gives 'sense.' Not as many words as letters can be grasped at one time, but three times as many letters when they make words as when they have no connection. Twice as many words can be grasped when they make a sentence as when they have no connection. The sentence is taken up as a whole; if it is not grasped, scarcely any of the words are read; if it is grasped, the words appear very distinct; this is also the case when the observer constructs an imaginary sentence from the traces he has taken up."[3]

"In a second series of experiments," to translate from the first article cited above, "the apparatus was still simpler, and the processes measured approach still more closely those of actual life. The problem was to determine the time which we need in order to read aloud letters and words, and to name colors and pictures of objects." Letters and words making, respectively, words and sentences, and secondly, lists of letters not making words, and lists of words not making sentences furnished the materials which were read in each of six languages, English, German, French, Italian, Latin, and Greek, familiar to the subjects.

Two of the findings from these experiments are of especial interest and importance. It took about twice as long to read aloud and as fast as possible words which had no connection as words which made sentences, and letters which had no connection as letters which made words. The explanation of this result is that "when the words make sentences and the letters words, not only do the processes of seeing and naming overlap, but by one mental effort, the subject can recognize a whole group of words or letters, and by one will-act choose the motions to be made in naming them. . . ."[4] This conclusion in regard to the recognition of groups of letters and words by a single act of consciousness was also supported by subsequent experiments of the author to be cited below, and was one of the first steps taken in the analysis of the mental processes concerned in reading.

---

[2] *Mind*, Vol. XI., No. 41, 1886, p. 64.

[3] *Brain*, Part XXXI., 1885, pp. 19–20.

A second finding of interest resulted from a study of the comparative rates of reading aloud in the six different languages employed. "The rate at which a person reads a foreign language is proportional to his familiarity with the language. For example, when reading as fast as possible the writer's [*i. e.*, Professor Cattell's] rate was, English 138, French 167, German 250, Italian 327, Latin 434, and Greek 484; the figures giving the thousandths of a second taken to read each word. Experiments made on others strikingly confirm these results. The subject does not know that he is reading the foreign language more slowly than his own; this explains why foreigners seem to talk so fast. This simple method of determining a person's familiarity with a language might be used in school-examinations."[5]

The reason for these differences may be found in the principle just enunciated. The recognition of groups of letters and words as wholes, and the choosing of the motions to be made in naming them take place the more promptly and completely in proportion to the subject's familiarity with the language employed. The present writer has recently elaborated this test of Professor Cattell's by combining it with a test which has been made use of to determine the ability of children in the early stages of instruction in reading to recognize word wholes. If the texts used are printed without spacing between the words as in the following illustrations, the recognition of the total word form is made more difficult and the differences between subjects proportionately emphasized.

Ithappenedonedayaboutnoongoingtowardsmyboat Iwasexceedingly
surprisedwiththeprintofaman'snakedfootontheshorewhichwas
veryplaintobeseeninthesand. Istoodlikeonethunderstruckorasif
Ihadseenanapparition. IlistenedIlookedaroundme. Icouldhear
nothingnorseeanything. Iwentuptoarisinggroundtolookfarther.
IwentuptheshoreanddowntheshorebutitwasalloneIcouldseeno
otherimpressionbutthatone. Iwenttoitagaintoseeiftherewereany
moreandtoobserveifitmightnotbemyfancybuttherewasnoroomfor
thatfortherewasexactlytheveryprintofafoottoesheelandeverypartof
afoot. HowitcamethitherIknewnotnorcouldnotintheleastimagine.

---

[4] *Mind*, Vol. XI., No. 41, 1886, p. 64.

[5] *Mind*, Vol. XI., No. 41, 1886, p. 65.

Ilétaitunefoisdeuxsoldats,quiavaientbiensoixanteans. Obligésde quitterleservice,ilsrésolurentderetourneraupays. Cheminfaisant ilssedisaient: Qu'allons-nousfairepourgagnernotrevie? Nous sommestropvieuxpourapprendreunmétier;sinousdemandonsnotre pain,onnousdiraquenoussommesencoreenétatdetravailler,etonne nousdonnerarien. Tironsausort,ditl'und'eux,àquiselaisseracrever lesyeux,etnousmendieronsensemble. L'autretrouval'idéebonne. Lesorttombasurceluiquiavaitfaitlaproposition;soncamaradelui crevalesyeux,et,l'unguidantl'autre,ilsallèrentdeporteenporte demanderleurpain. Onleurdonnaitbeaucoupmaisl'aveuglen'en profitaitguèresoncompagnongardaitpourlui-mêmetoutcequ'ily avaitdebonetneluidonnaitquelesosetlescroûtesdepaindur. Hélas disaitlemalheureuxn'estcepasassezd'étreaveugle? Faut-ilencore êtresimaltraité? Situteplainsencore,répondaitl'autrejetelaisserai là. Maislepauvreaveuglenepouvaits'empêcherdeseplaindre. Enfinsoncompagnonl'abandonnadansunbois.

EswareinmaleinalterKönigdernureineneinzigenSohnhattedenerſehrgern hätteheiratenſehen. AberderjungeMannkonntekeineBrautfindenundder VatergabihmendlicheinengoldenenSchlüſſelundſagte: „MeinSohngeheindas obereStockwerkdesTurmesundſiehedichdortumundſagemirwelchePrinzeſſindir dortambeſtengefällt." DerPrinzgingindasobereStockwerkdesTurmesöffnete einekleineeiſerneThürmitdemgoldenenSchlüſſelundkamineinZimmermitzwölf Fenſtern. AufjedesFenſterwardasBildeinerwunderſchönenPrinzeſſingemalt. ErtratſchnellzudieſemFenſterzogdenVorhangzurückundſaheineſehrſchöne PrinzeſſindieaberſoblaßundelendausſahalsobſieebenausdemGrabegeſtiegen wäre. DerPrinzſahſieeineZeitlangſchweigendanerbarmteſichihrerundrief laut„DieſeundkeineanderewillichzurGemahlinundſollteesmirmeinLebenkoſten!"

Under the title "Ueber die Trägheit der Netzhaut und des Sehcentrums," Professor Cattell published in the *Philosophische Studien* for the year 1885 the results of a series of experiments to determine the length of time that a visual stimulus must work on the retina in order that a sensation may be excited.[6] The experiments were begun in this country, but were for the most part carried out at Leipzig.

The gravity tachistoscope or chronometer, as constructed by Professor Cattell, was first made use of in these experiments.[7] The now familiar method is described in connection with the determination of the sensitiveness of the retina for colors. Light was reflected from colored surfaces, mounted on white cards. " . . . the card was placed on springs of the gravity chronometer, the observer fixated a point immediately before the center of the colored surface and allowed the screen to fall. The colored light worked on the retina for a period depending on the width of the slit in the screen. To determine how long the time must be taken, in order that a sensation may be excited, each of the several colors was taken separately, and, in addition, seven intensities of gray, including white and black. When the color is looked at for an extremely short time it appears gray, yellow approaching the shade of the white card, violet appearing black, the other colors of various shades of gray. Series were made in which the color to be investigated was placed about five times in the springs, shades of gray approaching in intensity the color about five times; the observer of course did not know in any single case whether the color or gray had been taken. After the light had worked on the retina the time desired, he decided whether he had seen the color or not. When the time was taken sufficiently long he always named the color, and when it was not there, said correctly 'gray.' On the other hand, when the time was taken very short, the observer saw no color, and either always said "gray," or if he imagined he saw color, was wrong as often as right. Thus it is easy to determine the time at which the color is usually seen; a point at which it is seen about nine out of ten trials. This point is quite constant, and can be determined to the ten-thousandth of a second."[8]

---

[6] The experiments were also reported in *Brain*, XXXI., pp. 2–20.

[7] Cf. Wundt's "Grundzüge der physiologischen Psychologie," 5th edition, Vol. III., p. 356.

The results showed that "light reflected from a colored surface must work on the retina from .6 to 2.75σ in order that it may be possible to see color. The time varies with the several colors and with the different observers; the order of the colors is, however, almost constant. The retina is most sensitive to orange light, but nearly equally so to yellow. Blue must work on the retina about .25σ longer than yellow, in order that it may be recognized. Next come red and green, red requiring about .1 and green .2σ longer than blue. The retina is least sensitive to violet light, the time for which is two to three times as long as for orange."[9]

"Substantially the same method as for colors was used to determine the time the light reflected from a letter or word must work on the retina in order that the letter or word may be distinguished." This time was found to be from 1 to 1.7σ and, except for long or rare words, to be slightly shorter for words than for letters. This observation that words are read as easily as letters had important bearings for the pedagogy of reading. The practise of teaching children to read words as words rather than as combinations of letters was already in common use, but this was the first experimental evidence tending to prove that words are read as wholes and to this extent gave justification for the change from the alphabet to the word method of teaching reading.

---

[8] *Brain,* XXXI., 1885, pp. 8–9.

[9] *Brain,* XXXI., 1885, pp. 9–10.

Another problem of very wide interest which was, so far as the writer is aware, first made the object of experimental investigation by Professor Cattell, concerned the question of the relative legibility of the letters of the alphabet, and of printing types. This study and those which have followed it, notably those of Sanford,[10] Dockeray,[11] and more recently of Roethlein,[12] have shown clearly the need of reform in printing, and have pointed out some changes which would make for improvement. Such reforms come slowly but because of the immense practical importance of this subject it would seem that in time the necessary support must be given for a more thoroughgoing and extensive study than it is possible for any one student or laboratory to undertake unaided.

The tachistoscopic method of experimentation has been applied to a variety of problems in visual attention with especial reference to the processes in reading. These studies which led to many different theories and conclusions call for no detailed review. They have been well summarized by several writers.[13]

Nearly all of the questions at issue, except in so far as the differences in the results were caused by variations in methods and apparatus (and this has been often the case) may to a very considerable extent be accounted for by the presence of significant individual differences in subjects. Whether subjects read words as wholes or in parts, the extent to which they make use of characteristic letter complexes as cues in reading, whether their attention is fixed to a limited or to a large part of the field of distinct vision, and the extent to which they are helped by the indistinct images of peripheral vision and by other premonitions of what is coming in the line—all these factors depend in the first instance on innate differences and secondly on habits of perception and recognition which often date back into early life and which are to some extent at least due to differences in methods of instruction or differences in the way in which the subjects themselves first learned to read as children.

An analysis of what children actually do, then, in learning to read, how they differ at the start in their ways of going about things, and the influence of the methods of teaching is very much needed

[10] *Amer. Journ. of Psychology*, I., 1888.

[11] *Journ. of Educat. Psychology*, I., 1910.

[12] Roethlein, "The Relative Legibility of Different Faces of Printing Types," Publications of Clark University Library, Vol. 3, January, 1912, No. 1.

[13] Cf. Whipple, "Manual of Mental and Physical Tests," pp. 222 ff.

to supplement the analysis of the adult processes. Among the initial contributions in this field, I wish to call attention to two studies made on classes of beginners in the elementary school of the University of Chicago.

In the first study[14] the pupils, who had had no instruction in reading, were taught by a word method without the use of phonics and the problem was to determine by what means the children actually recognized and differentiated words when left to their own devices. The following quotation indicates the methods employed by the experimenter: "First, incidents; for example, one day when the child was given the cards to read from, it was observed that she read with equal ease whether the card was right side up or upside down. This incident suggested a test which was later given. Second, comments of the child; for example, when she was asked to find in the context the word 'shoes,' she said that 'dress' looked so much like 'shoes' that she was afraid she would make a mistake. Third, questioning; for example, she had trouble to distinguish between 'sing' and 'song.' When she had mastered the words she was asked how she knew which was which. Her reply was, 'by the looks.' When questioned further she put her finger on the 'i' and the 'o.' These three types of evidence correspond to introspection with the adult. The fourth type of evidence is a comparison of the words learned with the words not learned as to the parts of speech, geometric form, internal form, and length. Fifth, misreadings; for example, 'dogs' was read 'twigs,' and 'feathers,' 'fur.' Sixth, mutilations; for example, 'dogs' was printed 'digs,' 'lilac' was written 'lalci.'"

Some of the conclusions may be cited, first as regards the kind of words most easily learned on the basis of the word forms. Four out of six children learned more "linear" words, *i. e.*, words like "acorns," "saw," in which there are no high letters, than of any other group. In but one case were the "superlinear" words more easily recognized. This is contrary to the finding of Messmer on adults; the latter considered that the "linear" words are less easily recognized because the contour is unbroken.

Misreadings or the mistaking of one word for another occurred most frequently in these early stages, first when the words were of the same length (which again controverts Messmer's findings); secondly, when words had common letters, the "g" and "o" of "igloo" caused it to be read as "dogs"; thirdly, when the initial letters of words were the same; and fourthly, when the final letters were the same. Words were recognized upside down nearly as easily as right side up, but two children noticing any difference. The word seems to be recognized as a whole, and as the author notes, recognized upside down just as the child would recognize a toy upside down.

---

[14] "Learning to Read," Josephine Horton Bowden, Elem. Sch. Teacher, Vol. 12, pp. 21–33.

The general conclusions of the study may be quoted:

"The comments and the questions, as well as the misreadings, seem to show that children learn to read words by the trial and error method. It may be the length of the word, the initial letter, the final letter, a characteristic letter, the position of the word in the sentence, or even the blackness of the type that serves as the cue. There were surprisingly few instances of learning by imitation. The first occurred in the fifth week, when one child pointed out the word 'the' and two other children pointed out other 'the's,' though similar opportunities had been presented before. Suggestion, which is a noticeable element in the reading of young children, also played a minor part. There is no evidence in any of the cases studied that the child works out a system by which he learns to recognize the words. That he does not work out phonics for himself comes out quite clearly in the transposition test. Furthermore, only once did a child divide a word even into its syllables. There is some evidence that the child is conscious of the letter, though there is none that he analyzes the word letter by letter, except in the case of "E," who so analyzed the word "six." Sometimes, when the child seemed to have made a letter analysis, he failed to recognize the word a second time, and in some cases did not learn it at all.

"Under the methods of instruction employed with this class as outlined above, it appears that these beginners in reading have after two months or more of instruction secured a sufficient conception of the general appearance of a very limited number of words to recognize them as wholes, that in doing this they made use of only very general cues or points of differentiation between words and have not noticed the finer points of distinction between words and parts of words. It appeared very doubtful to the experimenter whether, under this method of teaching words as visual wholes, the pupils would, of themselves, have come to make this latter necessary analysis with much success. Without some foregoing analysis and subsequent synthesis, the differences between words are not great enough to be recognized merely from the total visual appearance. The early introduction of phonics may supply, in some measure, this analysis. There is an undoubted advantage in having words presented at the start as units and wholes, as contrasted with the discarded teaching by letters. But that a word method can be used very long without some detailed analysis of the structure and parts of the words is altogether too common a notion in the theory if not in the practise of teaching."

Messmer, as a result of tachistoscopic experiments with a limited number of children concluded that children belong to the "subjective" type of readers. Results of Meumann, Freeman, and the present writer have controverted this statement, and one of the cases about to be cited will illustrate the fact that all the characteristics of an "objective" reader may be present in the first stages of learning to read. Even if these two types of readers as first differentiated by Messmer exist, it is a mistake to suppose that all readers may be classed under one type or the other. The writer has found in experiments made on nearly two hundred subjects that the majority are not to be classed in either type, these "types" representing rather some of the extremes of differences found among subjects. The chief differences between these types are said to be that the objective readers have a rather narrow span of attention in reading, but see accurately what they do see, and seldom guess or "read into" the material perceived, and that the subjective readers have a wider span, are influenced more by words lying in indirect vision, depend on relatively meager visual cues such as large word wholes, and that they are more likely to misread because of the large apperceptive element which they supply to the reading.

These are undoubtedly convenient rubrics, if they do not hinder the recognition of other combinations of individual differences, which may exist. The significance of the second study referred to above lies in the fact that it takes an inventory of some of these individual differences between children, and in part traces the effects of training on these differences.

The three subjects of the experiment had been learning to read for a year and a half.[15] To cite the findings in one case: Subject *B* could read in context 90.5 per cent. of the 1,588 words which appeared in the texts read. She read correctly 84.3 per cent. of the phonetic words, and 97.3 per cent. of the sight words in these texts. (Words which could be built up or worked out by sound were called "phonetic" words; words which could not be so worked out were called "sight" words.)

She could read but 50 per cent. of these same words when exposed singly and tachistoscopically, 31 per cent. of the phonetic words, and 70 per cent. of the sight words. This result is explained by the fact that over 40 per cent. of the words in the short exposures were seen only as parts of words. When given time to see the words read as parts or not seen correctly, 85 per cent. of them were made out or built up, 92 per cent. of the phonetic words, and 65 per cent. of the sight words.

---

[15] Sholty, Myrtle, A Study of the Reading Vocabulary of Children, Elem. Sch. Teacher, Vol. 12, 1911–12, pp. 272–277.

Subject *B* may on the basis of these findings be classified as an objective reader. The number of words, which were read in part only, in the short exposure experiments, indicates a narrow span of attention; what is read is read accurately, as indicated by the few misreadings. The effect of phonic drill on the methods of the objective reader is clearly seen in this case and in that of a second subject who might be similarly classified. The results show that the natural tendencies of the objective readers are reinforced by the phonic methods of instruction.

By way of contrast the performance of another pupil may be briefly cited. Out of the 1,588 words exposed tachistoscopically Subject *A* made but two incomplete readings, that is, read only two words as parts of words; she misread 182 words and did not recognize 427 at all, which shows that words were either read as wholes or not at all. Sight and phonetic words were read in context and singly in the short exposures in about the same proportion. Whereas *B* could "build up" or make out 26.6 per cent. more phonetic words than sight and another "objective" reader 38.5 per cent. more, Subject *A* could make out but 5.7 per cent. more phonetic than sight words. Finally, it may be added that *A* was selected as the most rapid and "best" reader in the class and read 88 per cent. of the words in context correctly.

Both *A* and *B* are successful readers but they make use of very different perceptual data in reading. *A* reads by large and only partially differentiated visual wholes and needs all the aids which the context supplies to supplement these relatively vague word forms and visual cues. The context stands in stead of the detailed consciousness of word elements which *B* has. *B* is disturbed by the tachistoscopic experiment because it interferes with the building up of words from their parts. *A* is chiefly disturbed because the words are out of their natural settings.

The phonetic analysis was suited to the methods of *B* and supplemented them; it was little adapted to and changed but little the methods employed by *A*,—which illustrates how differently those of different mental make-up may react to the same methods of instruction.

These examples have been cited to show the extension of the tachistoscopic experiment to the study of children's reading.

In 1900 Professor Cattell published the results of an extended study of phenomena connected with the perception of moving objects under the title "On Relations of Time and Space in Vision."[16] The chief results of the research are summarized as follows:

"When objects are moved slowly over a limited visual field, while the eyes are stationary, they are not seen one after the other at the place they are exhibited, but seem spread out over a larger area than can be seen. When successive white or colored surfaces pass over a limited field of vision so rapidly that the eye cannot be moved while they are in view, the stimuli do not seem to follow one another, but are perceived simultaneously side by side, variously arranged and commingled. What is physically a stimulus lasting a certain time and physiologically a series of changes in time is psychologically an area in which the colors are spread out side by side, and the processes in time are a spatial continuum. The colors appear in this case to cover a larger area than is exhibited."

Professor Cattell believed these findings to be paralleled by the processes which take place when the eyes move over a stationary field of objects, that although one object after another stimulates the same visual field, the objects are not perceived successively in a time series, but simultaneously side by side, that is, in a spatial continuum. Furthermore, since there is no fusion in the vision of daily life when the eye is in constant motion, this led to the assumption that "the visual organs respond to retinal changes more rapidly when the eye moves than when the objects are in motion."

This latter conclusion seemed to be rendered untenable by subsequent experiments of Professor Dodge indicating that there is fusion during movement of the eye in such instances, and clear vision only in the pauses which interrupt the movement. These findings, however, do not affect the main conclusion of the paper, in fact this possibility had been considered, for as Professor Cattell had noted, "if one had in fact only a few distinct images in the course of the movement, it would remain true that a series of successive images is seen as a space continuum."

The preceding section of this article drew attention to the very different way in which individuals may act to the same perceptual elements, and from the varied angle of these experiments we come back to the same general conclusion. To quote the words of Professor Cattell:

"These experiments show how absurd it would be to suppose that a perception is in any way a copy of the physical world or the correlate of a simple physiological process. The physical change is exactly the same and the physiological process is nearly the same for all observers, but the perceptions are entirely different. The artificial conditions of the experiment may perhaps give us some indication of what happens to each of us in learning to perceive the physical world as we do perceive it, and may also lead us to consider that there may be more individual variation in the perception of the same physical world than we commonly assume."

---

[16] Cattell, J. McKeen, "On Relations of Time and Space in Vision," *Psych. Rev.*, Vol. VII., No. 4, 1900, pp. 325–343.

To which may be added the final summing up of the paper:

"In general these experiments show that perceptions are not 'copies' of a physical world or the 'inner side' of special physiological processes, but are in large measure the result of experience and utility. The phenomena of vision as a whole require this conclusion, but the facts here described, showing that changes in time are perceived as extension in space, appear to be of fundamental importance for psychology and for epistemology."

Walter F. Dearborn

PROFESSOR CATTELL'S RELATION TO THE ASSOCIATION METHOD

Few attributes of Professor Cattell's work stand out more clearly than the capacity for seeing psychological methods in their distant potentialities. There was a time when every German medical dissertation must begin with the words, "*Schon Aretaeus sagte.*" And until time has fulfilled Watson's vision of an all-revealing laryngoscope, men shall surely find an origin of cherished conceptions with the personality in whose honor we are assembled.

With most of us, the development of the method of measurement by relative position stands out as his most signal contribution to psychological technique, as the concept of individual differences forms a broad foundation upon which most of the concrete values in experimental psychology are built. And yet from the special angle at which his work is seen by a psychopathologist he has sown no ideas which have flourished more, or give promise of a better harvest, than are to be found in the early papers connected with the subject of mental association. It is my privilege to point out the ways along which the ideas and methods here formulated have developed, and how their development has thrown into bold relief the value of the underlying *Fragestellung*, the shaping of psychology to meet the tests of action and life.

In their broadest definition, the association experiments are those which present a new situation, and measure the reaction to it as such. They deal either with the efficiency in making a prescribed, "controlled" reaction, or with the kinds of reaction that are most naturally made. The sorts of control may be greatly varied, according to the special ability it is desired to test, but it is safe to say that no broader series of controlled association methods have been introduced into any experimental study than are described in his paper of 1887; in fact, there are only three other procedures in the field that have any history, and two of these owe their development to his laboratory. In this first paper he brings out that while mental association has been the object of unorganized observation for centuries, it was his teacher Galton who first reduced its study to experimental form, and gave the impetus to its investigation by more refined methods in the laboratory of Wundt. To-day it could certainly be repeated with equal justice that "experimental psychology seems to have its most hopeful outlook in this direction."

Some of the simpler of these processes he had already dealt with; but though, as he remarks, they are continuous throughout, they do not usually receive the name of association until they reach the complexity of those described in this later connection. His use of the term "will-time" is especially prophetic of the more recent interpretations of these experiments.

His first further complication of his previous method was the naming in a foreign language of an object presented. The increase in the time over that necessary for the native language varied for the two subjects in proportion to their relative familiarity with the foreign language. By the same method, the translation time of short and long words was measured. Educational applications of these principles are also foreshadowed.

Next he presents measurements of the times taken to react to a stimulus word with the only response that stands in a determined relation to it. Examples of eight such varieties are given, and they are the foundation of the class to-day known as "controlled association tests," typifying those in which the control is the most rigid. "It will be noticed," he remarks in regard to them, "that the times of the two subjects correspond closely . . . ; the differences of time in the several cases are explained by the character and pursuits of the subjects, and in turn throw light back upon these . . . . For example, C knows quite as well as B that 5 + 7 is 12, and yet he needs 1/10 second longer to call it to mind; B knows quite as well as C that Dante was a poet, but needs 1/10 second longer to think of it." This opens the way for much interesting reflection upon what we mean by knowing a thing in relation to the ability to react to it effectively.

It also appeared that of these sorts of association reactions some were generally easier to establish than others; poets were easier to classify than men of science.

From these completely controlled processes, which may, as he says, be regarded as "recollection times," he passes on to the procedures where a varying amount of choice is involved, observing that the time is lengthened proportionally to the degree of choice. Thus a single city appears to have a closer mental connection with its country, than a month with its season, or an author with some special work. The association time is not, therefore, lengthened in proportion to the freedom of choice which the experiment objectively provides, but measures the extent to which certain particular associations have dominance over others which would satisfy the conditions. And occasional factors of suppression enter to complicate this interpretation.

Finally, he illustrates a series of processes whose complexity is beyond that of the ordinary conception of association experiments. These may be "controlled," in the sense of judging relative lengths of lines, or quite free, as in judging the relative eminence of men, or the pleasantness of colors. In these last, the time element loses its unequivocal import.

In speaking of its further evolution also, one can not fix precise limits as to what shall be called the controlled association method. Probably one should include here some of the more complicated choice reaction procedures, such as those employed by Gross in his study of epileptic depressions. Hahn attacked a very interesting problem in the relation between sound associations and false reactions, reasoning from the natural manic excitement to that produced by alcohol, but without reaching very positive results. Of the work of Henmon there is here no need to speak. What seems to be the only other large attempt to carry further the final suggestions of Professor Cattell's paper, is an investigation of Störring's, which observes the time required to draw proper conclusions about various syllogisms, an interest which he has carried further, into the pathological field. The controlled association tests have lately been the object of much effort, at the hands of various experimentalists, in the direction of standardization, which have been put to use from the educational side; August Mayer's work of some ten years ago is an initial step towards application of this nature. Not enough time has elapsed since this movement towards standardization began to know how well the present experimental material, as it is given by Whipple and others, will justify itself; but if it fails to do so, the reason will almost certainly be that in a natural desire for simplicity in handling, their more inaccessible features have been rated at far below their true importance. If a further refinement proves necessary, it must take the form of paying more heed to fluctuations, false reactions, and episodic blockings of motor adjustment.

A peculiar historical importance attaches to those experiments which deal with the arithmetical processes. Outside of Germany, they have only in special instances risen beyond the "mental test" level; but there, under the influence of Kraepelin, a special form of addition test was evolved which was taken to represent for the purposes of experiment—I had almost said to symbolize—the mental capacity of the subject. The extent of the *Psychologische Arbeiten* attests the manifold applications of the method, though it is impossible to speak of all the researches in which it has borne a part. It was early applied by Amberg, in conjunction with a memory method, to studying the effect of pauses on the capacity for mental work, while Rivers and Kraepelin made it the instrument of an investigation of fatigue and recuperation phenomena. Von Voss studied its minuter fluctuations. Weygandt employed it in observing the adaptation phenomena in change of work. Lindley made an analytical study of the effects of different rest periods, as well as of the spurt and practice phenomena. Some years later, Weygandt again used it among several other methods, in investigating the mental effects of hunger, the results seeming to differentiate them from those of the actual exhaustion psychoses. Studies along lines similar to Lindley's were also published by Hylan and Kraepelin and by Heüman. Perhaps the most conspicuous research of this type has been that of Specht, dealing with the comparative fatigue phenomena in normal individuals and in traumatic neuroses. Building upon the properties of the test as determined by previous investigators, the continuous and interrupted work-curves were so treated as to eliminate the factors of warming up, practise, etc., and to give the fatigue effect, as was thought, in a relatively pure form. But the results reported by Hutt, describing the experiments in cases of excitement or depression, and showing characteristic features of the psychoses without the statistical elaborations of the last-named study, are in many ways of greater significance.

Besides these researches, in which the experiment played a leading part, it has figured in many other investigations where the object has been a survey of psychic functions under particular conditions. It was one of the foundation stones of that groundwork of the Kraepelinian experimental studies, the *Beeinflussung einfacher psychischer Vorgänge durch einige Arzneimittel*, a field in which Kraepelin has always been interested, and which records a further use of the method in the investigation of bromides by Loewald, and of trional by Haenel. It was among the tests employed by Oehrn in the opening paper of the *Psychologische Arbeiten*, which bore the significant title of "Experimentelle Studien zur Individualpsychologie." It is found again in the territory of drug-psychology, in the researches of Hoch and Kraepelin on tea, of Kürz and Kraepelin on the mental effects of regular use of alcohol, and of Rüdin on the duration of alcohol effects. Miesemer used it, in observing with one subject, the comparative effects of bodily and mental effort, and Kasemann in a similar study concerning the obstruction of nasal breathing.

Among other researches in which the arithmetical type of controlled association tests has figured, are the investigations by Gilbert and Patrick on the loss of sleep, and by Weygandt measuring the recuperation during various periods of sleep. Among Professor Cattell's pupils they played some part in the studies of Marsh, Jones and Hollingworth, but have reached altogether their most elaborate development under Thorndike and his group, among whom the tendency has been to vastly increase the difficulty of the mental process so as to reduce the motor element, disturbing in the simpler tests, to a minimum.

Such methods have been *par excellence* the "intellectual" tests of psychology, because of their special adaptability to different functions of intellect, combined with simplicity of treatment. The readiness with which a person forms the requisite associations (*resp.* makes the adequate reaction) in such a test measures his general familiarity and facility in dealing with the material the test represents, compared with his facility in dealing with the material of other tests. To a considerable extent, though not so exactly, it measures his ability in these functions compared to that of other individuals. Thus the chief factor in the evaluation, the chief criterion of efficiency for these experiments, has been the association time, and the more closely controlled the association, the more entirely is this the case. Latterly, a very promising application has been made of these tests, suggestion for which is to be found in Professor Cattell's second paper on the general subject. This measures the constancy with which the associative processes are controllable, against their liability to interference through fatigue or shifting attention. The followers of Ebbinghaus with the *Kombinationsmethode*, and Yoakum with a non-linguistic method, have examined the matter from the former standpoint, Haggerty and Kempf from the latter.

Professor Cattell's paper of 1889 dealt with the subject of free association on the basis of something over 12,000 observations, which consisted mainly of a short series of responses from about 500 subjects, and a longer series from six subjects. Although it is the—*sit venia verbo*—discrete form of the association method that is here employed, he has also many observations with the continuous method, extensive work with which has since been done by Jastrow, and, with pathological material, by Town.

In regard to the association time, he presents figures for himself and his co-worker Berger that analyze out the association time from the perception and movement time, which were known for both. They are very much shorter than is the rule with ordinary subjects. There was a four-fold division of the stimulus words; verbs, and nouns of three degrees of concreteness. The more abstract the noun, the longer the time; a point which later investigators have also considered with generally the same results.

Upon this basis, further experiments with classified stimulus words were made. While confirming the previous finding, they showed a wide range of individual variation, indicating, as the authors remark, "important individual differences in rate of thought and stage of mental development." The use of abstract thought in education is observed to quicken its relative rate in association time.

In the experiments by the continuous method "distinct shortening of the mental process" accompanying growth and education was shown. And only recently these applications of the general method have been rediscovered, Rosanoff being the principal mover in this direction.

There was a general though not marked shortening of the association time as the class rank of the students was higher. "Indeed," it is remarked, "it is possible that such experiments measure the alertness of the student's mind more accurately than does the class-rank, which depends largely on diligence and other factors not telling in such experiments." Here again we have a forecast of the far-reaching interpretations which have since been put upon the method. The authors also observed that if the starting-point in such a continued series is a concrete noun, considerably more associations are made to it than if it began with an abstract noun. Especially interesting is the finding that the ease of forming such associations decreases as the experiment progresses, which, as is remarked, "bears witness to, and in a way measures, fatigue or decrease in attention."

Next comes an empirical study of the nature of the associative process. On these grounds they reject the rationalized categories of contiguity and similarity, replacing them with the general distinction of outer, or objective, and inner, or logical, associations, which has borne a very prominent part in later studies. The complete scheme of classification was as follows:

Association			
Objective		Logical	
Co-existence	Succession	Specification	Causation
Co-ord, Wh. to Pt.	Pt. to Wh.	Cor. Spec. Gen.	Fin. Effic.

Among these, the group of specification corresponds quite closely with those categories which under the names of predicate and egocentric, have played the most conspicuous subsequent rôle.

The writers are more alive than some later workers have been to the sources of error involved; "We do not," they say, "look upon the classes as 'natural kinds.' On the one hand, our subdivisions run into each other, and an association is rarely or never due to one only of the relations; on the other hand, further subdivisions might be made," as the paper goes on to illustrate.

The authors give an analysis of the totals of their material according to these divisions, which afford averages upon which to base the interpretation of the subsequent data. It is noted that "the nature of the association differs considerably with different persons and classes of persons"; that is, there is shown a tendency towards association types. Logical associations appear to be especially favored in those persons of literary environment. These indications in regard to the effects of intellectual training do not correspond with those reported by Jung, and, indeed, the intellectual factor probably has a far from dominating influence on the association type.

Some data are also presented concerning the influence of the stimulus word on the form of the association. This is regarded as considerable; one word favoring whole-part associations, another specializations, another those of final cause. This is also borne out by later studies; but this factor does not necessitate a larger series of stimulus words than would otherwise be required for a single measurement of association type. Fifty words is a minimum for this purpose. Finally, encouraging stress is laid upon the quantitative aspects of the method, both in respect to the measurement of mental time, and the nature of its process, which its subsequent history has amply justified.

Professor Cattell was thus a pioneer in the construction of a systematic method for the classification of associations on the basis of the relations of the responses and the stimulus words. This idea has since developed along two lines; first the representation, as accurately as possible, of the logical relations thus implied, and secondly, the use of this standpoint in classification as a means to the end of studying particular mental mechanisms in the subject. Psychological progress in the first of these directions is linked inseparably with the names of Münsterberg, Aschaffenburg, Wreschner, and Watt.

Jung seems to have been the first to suspect a correlation between certain types of personality and certain sorts of association which were readily susceptible to definition along the established lines. Although the methods by which he worked were very much under grammatical influence, he made a distinction of three sorts of association type, that he called the egocentric, the predicate, and the concrete, and which he believed to be conditioned by pervasive individual differences in mental reaction type. It was possible to show, however, that the predicate and egocentric types of association are so closely allied that the main line of cleavage is between them and those of his *Sachlicher Typus* or concrete type. Since the relative tendency to these two groups of association seems to be the essential thing from the psychodiagnostic standpoint, it is possible to eliminate many subdivisions which do not serve these purposes, and as a matter of fact, these finer definitions are largely impossible to preserve in practise. That these two associative categories represent deeplying differences in mental makeup is borne out by the personalities that show them most typically.

As the criteria of classification became better fixed, they made firmer ground for those quantitative determinations by which the experiment was to establish itself as a scientific method. The first comparative study of strictly *individual* differences in association type is that of Fürst, whose results showed the tendency of the association type to correspond among persons of the same sex and family. This was one of the remarkable series of researches under Jung's direction which make up the *Diagnostische Assoziationsstudien.* Since its appearance, further investigations have emphasized the validity and manifold character of the individual differences of which this experiment is a function, and its data have proved themselves not unworthy of the most refined statistical treatment.

Those who have sought its correlations in the intellectual sphere have been meagerly rewarded beside those who have applied it to the study of affective and volitional processes. But it has never been possible, and there is no evidence that it ever will be possible, to evolve a practical system of classification that is not greatly dependent upon the personal equation of the classifier. Strictly objective methods have been attained only where the given associations have been taken at their face value, and correlated directly with other measurable features of the personality.

Professor Cattell took the first step in this direction when he printed complete lists of the responses of 465 subjects to certain stimulus words. Though working with very moderate material, Saling in 1908 saw far into the theoretical bearings of this method, even compiling an exemplar of a "dictionary of associations." Bovet in 1910 made a brief note of the same nature, and the same year Reinhold published a complete lexicon of this character from the results of three hundred subjects.

The most conspicuous development in this direction, however, has been the work of Kent and Rosanoff, which classifies the responses to 100 stimulus words by a thousand subjects, providing a very fine scale measure of a person's tendency to usualness or individuality in associative responses. Owing to the fact that it is the most fully worked out of association tests, probably indeed the most standard of all psychological experiments, it lends itself much better than any other form to objective and quantitative treatment. Its very indispensability makes it a matter for lasting regret that the more intimate *Anhaltspunkte* of mental diagnosis had so little part in determining its content.

No defense need be offered here of the general doing away with introspection that has prevailed in these researches. The time that might be so used is, we think, better spent in increasing the number of observations. The case is different when the object is a thorough analysis of single personalities without regard to precise values for comparison. This is the problem encountered in clinical examination, where the experiment has done no small service, not as a measure of reaction type, but as an indicator of particular emotional constellations. Here it forms a *point d'appui* for the minutest introspections, its use in this respect being analogous to that of the dream.

The entire *Komplexlehre* is implicit in Professor Cattell's sentence, "Such experiments lay bare the mental life in a way that is startling and not always gratifying." So startling and so ungratifying in fact, that subsequent workers with association experiments seem long to have been effectually blocked for any progress along this line. One can not help thinking that the literature of the "thought processes" would fare worse at James's pen than did any of the Fechnerian lucubrations. Not for many years was this bearing of the experiments appreciated; and its reintroduction came not from within experimental psychology, but from clinical and judicial angles, now inseparably united with the names of *Tatbestandsdiagnostik* and psychoanalysis. The first of these sets the definite experimental problem of how far it can be determined by association tests whether a certain knowledge exists, which may imply the performance of a certain act. Its technique has been threshed out from juristic sources; there are also some laboratory studies, in which Washburn, Yerkes and others have taken part. This work has, in fact, an extent which few of us realize until it is collated by some skillful reviewer like Rittershaus; though there seems no escape from his conclusion that the distinction of innocent from guilty knowledge remains an insuperable difficulty in judicial application.

The *raison d'être* of the method was the fact that the presence of special affective coloring would make characteristic alterations in the associative process. Obviously this would apply to all sorts of experiences, whether or not of criminal nature; and a long and profitable series of investigations have had for their object the study of how emotional reactions in general were manifested in the association test. On account of its clinical alliances, this inquiry was always close coupled with another; what form of the experiment was best suited to the determination of what, if any, particular emotional constellations were present in the subject, or, more broadly, what was the general tendency of the subject in respect to intensity of emotional reactions?

All such questions have to-day their best, almost their only experimental answer in the association test combined with some form of the method of expression; the electrical reactions are best understood in this relation, though we can not say they are the most accurate until others have also been tested with thoroughness. It is measurement by relative position that has made such testing possible. Recent work also indicates that we may yet turn for mental diagnosis from the free association procedures involved above, to those of controlled association with which Professor Cattell's original observation was made.

There is actually no psychological method whose pathological relationships have been so important. Aschaffenburg's material on the manic flight was the first systematic work, and our knowledge of the manic-depressive psychosis in this respect has since been considerably extended by Isserlin. Results of exceptional interest were obtained in dementia præcox cases by Pfenninger, while defect conditions have been studied by Wehrlin, an earlier follower of Jung, and more recently by Eastman and Rosanoff. Among the observations covering a wider range of psychoses, made with reference to finding differential symptoms, the material of Kent and Rosanoff is again preeminent. It has become practicable also to make more precise studies of change in reaction type with the progress or remission of mental disease, and the cases observed have shown striking reflections of such changes, as would scarcely have been possible save with an experiment capable of analysis from many different angles. Birnbaum has also studied the way in which the experiment reflects differentially a general mental symptom like emotional depression, when it is the concomitant of different psychotic states.

Almost every direction in which the association experiment has since developed is pointed out in Professor Cattell's brief contributions, and he who wishes a problem of research in this field may still turn to them for suggestion and guidance. Underlying all, however, is the insistence on contact with reality. "The mental processes considered above," he writes, "are by no means invented for the sake of the experiment, but are such as make up a considerable part of life." Especially does this appear in his paper of 1896 on the accuracy of recollection, where he discusses the "errors of observation" in daily life, the probable error of judicial testimony, and makes a constructive criticism of the sorts of intelligence tests calculated to determine efficiency for definite kinds of work. From this pragmatic angle his contributions to dynamic psychology have an added power of inspiration. "If we can disentangle the complex influences of heredity and environment, we may be able to apply our knowledge to guide human conduct."

Four men, it is said, stood with God on the morning of creation, surveying the work. "How did you do it?" said one; "Let me have it," said another; and a third, "What is it for?" From the outlook on the conditions which produce the efficient worker in science, one of our leader's dominating interests, it is a clear way to the point whence it seems that the ultimate aim which a viable psychology must set itself is the broadening of this problem to include not only men of science, but all other individuals, to produce well balanced men, as well as scientific ones. The human psychology of the future is the science of the mental adjustment of human beings to the environment. In formulating the genesis of the scientific investigator as his essential personal problem, Professor Cattell has created its paradigma.

It is a patent fact that not every one interested in psychological questions takes this view of scientific responsibilities. Many a philosopher, when confronted with an unpalatable datum of human experience, stands ready to remind you that a thing is not truth because it "works." It is a healthier view that a thing is not truth unless it "works." The psychology of to-day is very much a science of looking for the truth and hoping you won't find it. What else can be said of a body of knowledge so untranslatable into conduct? One knows his psychology as a person knows typewriting who has learned a chart of the keyboard. How often do we perceive a person's training to proceed in negativistic disregard of the facts of mental adaptation! On the social side, we know that certain sorts of individuals should not become parents, that the control of alcohol and syphilis would immensely lighten the burdens of mental disease, that failure of adequate segregation makes the correctional institution a university of iniquity. There follows however, but a half-hearted attempt to determine, or seek to be guided by, the courses of action to best obviate these misfortunes, and achieve their opposites. The means of increasing our psychological knowledge are certainly no greater matter for concern than the difficulty of making that knowledge effective in action. I have forgotten if it was a psychologist or a clergyman who gave friendly greeting to the signboard he passed upon the road. Because, he said, it is a colleague of mine; it also points the way and doesn't travel it.

Against this folding of scientific talent in the napkin of wilful inconsequentiality, Professor Cattell's followers have bent untiring energy; and as we gain fuller comprehension of how his conception of adapting individual and environment in science irradiates over the rest of human activity, we may look for some of the most influential researches that his insight has inspired.

Thus because a certain kind of work is of the greatest social service, it is not necessarily of the greatest social service to have a given individual enter upon it. You can not make a silk purse out of a sow's ear, but you can make a leather one and have more to put into it. A concrete application of mental adjustment must see to it that not only are the right persons attracted to, let us say, scientific work, but that the wrong ones are kept away from it. It is of more service to society to place wise legislation on the statute-books than to write grammars of extinct languages, but there will always be individuals who will preserve a better balance, and make mistakes of less consequence in occupying themselves with the latter. And there is a real danger in the over-idealization of any variety of work in that unfitted individuals may be drawn into it before their sense of personal values is properly matured. There are limits to the self-sacrificing capacity of finite human nature even in the "service of society," when that service is one whose results the individual must principally imagine. Indeed, this very expression is, like all names of virtues, easily dissociated from the activities it symbolizes, and serves but as the formula of a sanction too vague for statement in terms of actual conduct. It is more than probable that there are to-day enrolled among the workers in science, for idealistic reasons, many who would be of greater "service to society," as well as to those nearest them, and with a healthier satisfaction in themselves, in the pursuits of profession, industry or agriculture.

Even where this general direction of activity is determined, it is unwise to treat its followers as a distinct temperamental group.

A character of such marked individualism as Professor Cattell's would surely discountenance the assumption of a definite environmental optimum for the productivity of such tremendously variable personalities as are found among scientific men. His question, of whether the progress of science would be furthered more by assured competency only remotely dependent on "results," or by severe competition for large prizes, is the question of which of these is the most suitable for different persons, for some would do their best work under one condition, some under another.

Related lines of investigation are, for example, what sorts of activities in adolescent life are associated with the best balance of the personality in later years. It would be most significant to know whether, in the great universities, Phi Beta Kappa men or football men would show up better in this regard. Though the former may have the better chance of "Who's Who," this group, as Professor Cattell was quick to point out, favors "those who talk about things rather than those who do them," and is an equally unreliable criterion of mental balance and adaptation. It would be important to learn the complete meaning for after life of the clear distinctions between college and technical students, groups representing special abilities or ineptitudes, various sexual habits. Do the honor men in literature or the honor men in mathematics prove the better personalities? What becomes of those whom their classmates designate respectively as the "most popular," the "least appreciated," the "most suburban"? Here is a field in which the value of all early determinations accumulates at usurious interest, if we will adopt the psychiatric procedure of "following the cases," tracing through after life how capable such men have been in positions, how they were esteemed by their fellows, how much money they earned, how rapidly they advanced, at what age they married, what sort of marriages they made, what sort of children they reared.

The last enemy which shall be destroyed is, in fact, the tendency to unfit matings on the basis of sentiment. It is a not uncommon remark that instinct is a safer guide to marriage than reason, and there is no doubt that it is a surer one. But it is one thing to be guided by instinct, and quite another to be guided by a series of false conventions—or lack of them—masquerading as instinct. Social evolution depends moreover, not on the following of such instincts to their immediate conclusion, but upon their adjustment to other social forces, and this is not given in the undisciplined emotion of

> L'amour est enfant de Bohème
> Qui n'a jamais su de loi,

but through reaction in accordance with the broad experience of the race. That education is far from powerless in sexual control, is anthropological history.

Prerequisite to an effective psychology of mental adaptation is the knowledge of mental mechanisms. Upon principles which Professor Cattell enunciated have been built the most exact methods for their understanding that exist. Advancing from this point, he has formulated a series of psychological problems unique in their range of contact with life, and always close-linked to those instruments of objective method which are the most distinctive of his achievements. Every honor then, to the armorer of constructive experimentalism; whose fairest token is the unswerving progress of the weapons he forged to our hands.

F. LYMAN WELLS

PROFESSOR CATTELL'S PSYCHOPHYSICAL CONTRIBUTIONS

In his address at the International Congress of Arts and Sciences, St. Louis, 1904, on "The Conceptions and Methods of Psychology,"[1] Professor Cattell made some remarks that are of interest in connection with recent discussions of introspective and behavior psychology. "The task has been assigned to me," he says, "of considering the scope, conceptions and methods of psychology, and it is my business to define the field of psychology or to acknowledge my inability to do so. I must choose the latter alternative. I can only say that psychology is what the psychologist is interested in *qua* psychologist. If it is said that this is tautological, it may be replied that tautology is characteristic of definitions . . . . I am not convinced that psychology should be limited to the study of consciousness as such, in so far as this can be set off from the physical world . . . . There is no conflict between introspective analysis and objective experiment—on the contrary, they should and do continually cooperate. But the rather widespread notion that there is no psychology apart from introspection is refuted by the brute argument of accomplished fact.

"It seems to me that most of the research work that has been done by me or in my laboratory is nearly as independent of introspection as work in physics or zoology. The time of mental processes, the accuracy of perception and movement, the range of consciousness, fatigue and practise, the motor accompaniments of thought, memory, the association of ideas, the perception of space, color-vision, preferences, judgments, individual differences, the behavior of animals and of children, these and other topics I have investigated without requiring the slightest introspection on the part of the subject or undertaking such on my own part during the course of the experiments. It is usually no more necessary for the subject to be a psychologist that it is for the vivisected frog to be a physiologist. . . .

"Let us take a broad view and be liberal in our appreciation; let us welcome variations and sports; if birth is given to monstrosities on occasion, we may be sure that they will not survive. . . . As I claim for psychology the freedom of the universe in its subject-matter, so I believe that every method of science can be used by the psychologist. The two great achievements of science have been the elaboration of the quantitative method on the one hand and of the genetic method on the other. . . . It would be an irreparable limitation if either of these methods did not apply in psychology. In my opinion they not only do obtain but must obtain."

---

[1] Published in the report of the Congress, and also in the *Popular Science Monthly*, December, 1904.

These quotations express what must have been felt by every one who has worked with Professor Cattell: his catholic temper towards all varieties of psychological interest, and at the same time his personal predilection for the more positive sides of the subject, for the study of behavior or conduct, for the use of objective and especially quantitative methods. It is not strange, accordingly, to find these tendencies in his work in psychophysics, and to see him cut loose from the rather sophisticated conception of this branch as a measurement of sensations, and treat it as a study of certain phases of human behavior. There is certainly an element of sophistication in regarding experiments on the just noticeable difference, or on the average error, or on the per cent. of error in the method of right and wrong cases, as measurements of sensation. The subject of the experiment is not assigned the task of measuring his sensations, and does not seem to himself to be doing any such thing; and it required the ingenuity of a Fechner and of a Müller to make such an interpretation anywise plausible. Taken at their face value, these experiments are concerned with errors of observation, and this conception of the matter dominates the whole of our author's work on psychophysics.

The principal work of Professor Cattell in the present field was made in collaboration with Professor Fullerton, then his colleague at the University of Pennsylvania.[2] Some of the main conclusions were reported by Professor Cattell at the 1892 meeting of the American Psychological Association, and printed soon afterwards.[3] Reference should also be made to the paper[4] in which he introduced the methods of serial arrangement and of discrimination time as means of studying the perception of small differences, though these two papers are more fully considered by other contributors to this volume.

This work has many points of interest in the matter of method.

[2] Fullerton and Cattell, "On the Perception of Small Differences," Publications of the University of Pennsylvania, Philosophical Series, No. 2, 1892.

[3] Cattell, "On Errors of Observation," *American Journal of Psychology*, 1893, 5, No. 3.

[4] "The Time of Perception as a Measure of Differences in Intensity," *Philosophische Studien*, 1902, 19, 63–68.

The method of mean gradations, or of "equal sense distances," is subjected to important criticisms. This method, unlike the error methods, seems to require the subject to estimate the magnitude of his sensations, and therefore has been accepted as affording a direct measure of sensations. Yet it is by no means certain that the subject accomplishes the task that is set him. He is instructed to establish equal sensation differences; but every one will admit that this is an unaccustomed task. Our whole tendency is towards taking sensations as the signs of objective facts, and our quantitative judgments, especially, refer habitually to objective magnitudes. It is too much to expect the subject to lay aside this universal tendency at the mere command of the experimenter; and those psychologists who believe this to be possible at least admit its difficulty, and warn the subject against the "stimulus error," or tendency to judge in objective terms instead of terms of sensation. No doubt *some* stimulus errors can be avoided as the result of warning; but, in avoiding them, the subject may simply revert to more primitive forms of objective perception. In the words of Professor Cattell:[5]

"The question here is whether we do in fact judge differences in the intensity of sensations, or whether we merely judge differences in the stimuli determined by association with their known objective relations. I am inclined to think that the latter is the case. I find it comparatively easy to adjust one time, length of line or weight midway between two others, much more difficult to judge when one light or sound is midway between two others, and almost impossible to judge one temperature or pain midway between two others. The difficulty of making a decision increases as the objective relations are less familiar, and I believe that my adjustment is always determined by association with the known quantitative relations of the physical world. . . . I conclude, consequently, that we can not measure the intensity of sensation and its relation to the energy of the stimulus either by determining the error of observation or by estimating amounts of difference."

It would be going much too far to say that this rather revolutionary conception of psychophysics has been universally adopted, for it has met with strenuous opposition; yet it was fairly prophetic of the later course of investigation. Of those who have since employed the psychophysical methods, some, as Bentley, Whipple, and Martin and Müller, though making much use of introspection, have been concerned with the *modus operandi* of the judgment, while others, as Urban and his pupils, interpret the methods as measuring the probability of a given judgment under different conditions, the sensitivity of different individuals, etc. Measurement of sensation has receded into the background, and the whole subject of psychophysics has assumed a more positive aspect than formerly.

---

[5] *Amer. Journ. of Psychol.*, 1893, 5, 293.

Of the common psychophysical methods, Fullerton and Cattell favor that of average error, provided adjustment of the stimulus by the subject is practicable, and otherwise the method of right and wrong cases. Average error has a great advantage on the score of economy, because each single trial gives a measurement, whereas the method of right and wrong cases simply counts the trials that fall in different classes, and requires a large number of cases to determine the relative frequency of the classes. In his tests for individual differences, accordingly, Professor Cattell has always striven to introduce adjustment, so as to get a measure with the least sacrifice of the subject's time.

There is, however, a difficulty in the method of average error, which is pointed out by Fullerton and Cattell, and emphasized by the results of their experiments. It is connected with the time occupied by the subject in making his adjustment. If he is allowed unlimited time, and is free to adjust and re-adjust till fully satisfied, he may in effect make several trials, only the last one of which is considered, so that the procedure becomes inconstant. On the other hand, if his time is limited, he may be able, after making the best adjustment possible in the alloted time, still to recognize his error, so that his full power of perception is not measured. The time limit is almost unavoidable in studying the perception of active bodily movement. "The observer is required to make one movement and then another movement as nearly as possible equal to it, the error in extent, force or time being measured by suitable apparatus. In this case, however, the error is complex, being partly an error of perception, and partly an error in adjusting the second movement. We do not see how the error of adjustment can be eliminated otherwise than by using the method of right and wrong cases."[6] They accordingly superimposed upon the adjustment a judgment by the subject of the direction of his error, and found that these judgments, though made with little confidence, were right in a considerable majority of the cases.

On the side of method, the most important contribution of Fullerton and Cattell is a criticism of the method of just noticeable differences, and, less explicitly but no less really, of a common way of using the method of right and wrong cases. It is essentially the same criticism that is applied to both of these methods, and the basis of the criticism is found chiefly in the results of several series of experiments. In these experiments, made by the method of right and wrong cases, the subject was instructed to decide in each case whether the second magnitude presented was greater or less than the first, excluding as far as possible "equal" and "doubtful" judgments. This exclusion of the middle class of judgments had been adopted by a few previous authors and defended most completely by Jastrow.[7] Fullerton and Cattell instructed their subjects also to assign a degree of confidence to each judgment. The judgments were to be classed as *A*, *B*, *C* or *D*, *A* meaning sure of the correctness of the judgment, *B* meaning fairly confident, *C* uncertain, and *D* a mere guess. Introspectively, "the confidence seemed to correspond exactly with the apparent amount of difference. Thus the observer said *A* if the difference seemed quite evident, *B* if the difference was apparent but not so great that he felt sure of the correctness of his judgment, *C* if he noticed but little difference and felt doubtful as to his correctness, and *D* if he could discern no difference, and felt that his judgment was a mere guess."[8] These introspections were confirmed by the objective facts, for, as the difference between the stimuli increased, the number of confident judgments, as well as the number of correct judgments, increased also. "The confidence thus varies nearly as the percentage of right cases, and some reliance may, therefore, be placed on such introspection."[9] The confident judgments were more often correct than the less confident. In experiments with lifted weights,[10] the most confident or *A* judgments of one subject were correct 93 per cent. of the time, the *B* judgments 86 per cent., the *C* judgments 74 per cent., and the *D* judgments, or mere guesses, 65 per cent.; and all the subjects showed the same general fact. It was thus established that judgments can be arranged with some success along a scale of confidence or certainty.

---

[6] Fullerton and Cattell, pp. 18, 19.

None the less clear, however, is the result that individuals differ greatly in their graduation of the scale of confidence. "Different individuals place very different meanings on the degree of confidence. Some observers are nearly always quite or fairly confident, while others are seldom quite confident."[11] Some subjects put as many as 70 per cent. of all their judgments into the *A* class, and some as few as 6 per cent. Nor were the more confident subjects necessarily the more correct, but, if anything,

[7] *Amer. Journ. of Psychol.*, 1888, **1**, 277, 305.

[8] *Op. cit.*, p. 132.

[9] P. 126.

[10] P. 132.

[11] P. 126.

the reverse, since the most accurate observers were chary with their *A*'s, while some of the least accurate observers were very free with them. The standard set by an individual for the admission of a judgment to class *A* is indicated by the per cent. of erroneous judgments admitted. Some subjects admitted no erroneous judgments to this class, while some admitted 10 per cent. or even more; and the standards for the other classes varied similarly. As long as we confine our attention to a single individual, therefore, we can use the confidence of a judgment as a fair indication of its correctness; but, as between different individuals, we can not do so. This individual difference in confidence is doubtless temperamental in part, and in part merely a chance variation such as would be expected in the process of dividing a continuum into a series of compartments without objective aid or control.

This study in the psychology of confidence has an important bearing on the method of just noticeable differences, and also on that common form of the method of right and wrong cases in which a middle class of "equal" or "doubtful" judgments is allowed between the judgments "greater" and "less." This middle class of judgments is present also in the method of least noticeable differences, which, in fact, has for its object to determine the boundaries of the middle class. The difficulty with both methods is the same, namely, that the boundaries of the middle class are fixed by each subject without objective control, and almost inevitably differ from subject to subject, just as the boundaries of the several classes of more or less confident judgments differ. Whether a given pair of stimuli shall be classed as equal or different depends on the subjective standards of clearness or confidence which the observer sets up as boundaries. If he is cautious and insists on a clear difference in order to give the judgment "greater" or "less," he will leave a broad middle zone and be ticketed as having a large threshold of difference or an inferior power of discrimination; while if he is willing to take chances and follow the lead of slight impressions, he may make occasional errors, but the method will give him a low threshold of difference and a high power of discrimination. In the experiments already alluded to on lifted weights, for example, the standard weight being 100 grams, one observer gets a threshold of 6 grams and another of 13 grams, if the *C* and *D* classes are combined to give the middle class; yet when the accuracy of all the judgments is taken into account, it is found that the observer with the larger "threshold" is a much finer observer than the other. There are other instances of this discrepancy in other parts of the paper. The method of just noticeable differences can be relied on to give many glaringly false results if it is applied to the measurement of individual differences. Even if the comparisons are confined to a single individual, there is no guarantee that his subjective standard does not become changed in passing from one set of conditions to another. In fact, the method of least noticeable differences has perhaps never been highly recommended for accuracy, but it has won favor from seeming to afford a direct measure of sensation.

As already suggested, the method of right and wrong cases is open to this same objection, when a middle class of equal or doubtful judgments is permitted. Fullerton and Cattell do not criticize this procedure so explicitly as they do the method of least noticeable differences, but the criticism is implied in their rejection of the procedure, in their discovery that the doubtful judgments can be assigned to the "greater" and "less" classes with a clear balance of correctness, and in other incidental remarks and findings. Since the exclusion of the middle class has been severely criticized by Müller, Titchener and others, it may be well to point out somewhat carefully the bearing of the results of Fullerton and Cattell.

The main point is, of course, that the middle class of judgments (consisting of "equal" and "doubtful" judgments) has only a subjective definition, varying from individual to individual. One individual, insisting on a rather high degree of clearness or confidence for his "greater" or "less" judgments, will throw a large proportion of the cases into the middle class, while another individual will follow slighter indications and throw similar cases into the plus or minus column. The individuals differ in their standards of confidence, but not necessarily in their power to perceive a difference. Now, since it has been proved that the division of the middle class between the other classes results in a good balance of correctness, the individual who takes the more chances, or has the lower standard of clearness, will get the better record. The older method of Fechner divided the middle class equally between the extreme classes in calculating the final per cent. of right judgments; while the observer himself, by guessing, would have got more than half of the middle class right and thus have raised his record. By the newer methods of Müller, and of Urban, the middle class is left intact and treated separately, but the principal interest attaches to the points at which the "greater" and the "less" judgments, respectively, cross the 50 per cent. mark. Urban's "interval of uncertainty"[12] is the interval between that stimulus which is judged greater than the standard stimulus 50 per cent. of the time and that which is judged less 50 per cent. of the time; and these points are determined without reference to the equality judgments. But the subject, if he only knew it, has it in his power to lessen this interval by simply using his best judgment in the presence of each doubtful case, and throwing it into either the plus or the minus class. By so doing, he will raise the per cent. of plus and minus judgments at every point; and, if he throws *all* the doubtful cases into the other columns, will reduce the interval of uncertainty, and the threshold, to zero, and come out with infinity as the measure of his power of discrimination. Following the spirit of his instructions, the subject is not likely to go as far as this, but how far he shall go in this direction depends on the subjective standards of confidence which he sets up, and not, in the main, on his sensory discrimination. Urban finds, in fact, rather extraordinary differences between his subjects, one of them showing five times the accuracy of discrimination of another. Ordinarily, as Professor Cattell has often said, the range of difference between normal adults in such matters comes out in about the ratio of 1 to 2. It is, I believe, rather probable that the differences here reported by Urban are differences in subjective standard and not in sensory discrimination; and it is by no means impossible, from the indications given by Fullerton and Cattell, that a proper measure of sense discrimination would completely alter the arrangement of these individuals. It is precisely the excellent work of Urban in giving the "method of constant stimuli" an intelligible meaning and in providing facilities for its use, and the consequent probability that the method will be much in demand for serious investigations, that make the preceding criticism necessary.

---

[12] *Psychol. Review,* 1910, **17**, 235.

The motive behind the exclusion of the middle class of judgments has been often understood to be simply the desire to avoid the old difficulty of dealing mathematically with these judgments; and since a formally perfect manner of handling them has been devised, it has been urged that the motive has lapsed.[13] But this is to overlook the main point—which has, indeed, perhaps not been sufficiently emphasized. The real difficulty with the middle class is not mathematical, but psychological. The method of Müller and of Urban, though formally perfect, has the psychological defect of overlooking what might be called the "error of uncontrolled subjective standards," already sufficiently described. When any considerable number of "doubtful" cases is permitted, we know positively, from experimental results, that a full measure of the "differential sensitivity" has not been obtained, since the doubtful cases, as they stand, give no evidence of sensitivity, while a division of them by the subject would reveal, in the balance of right judgments, a degree of sensitivity lurking among them. We also know that the definition of the middle class is without objective control, and varies from individual to individual and probably in the same individual under different conditions.

---

[13] Titchener, "Experimental Psychology, Instructor's Manual, Quantitative," 1905, p. 290.

It has been objected by Müller and others that the exclusion of doubtful cases puts undue compulsion on the subject, does not allow him to follow his direct impression when this is one of complete equality or uncertainty, and so trains him to be careless and unconscientious in his responses. These difficulties are not encountered in actual experiments, at least with practised subjects; and the plan of assigning a degree of confidence to each judgment satisfies the conscience of even the inexperienced subject. Warner Brown,[14] who among recent authors has given the plan of excluding the middle class its most thorough test, found positive impressions of equality very rare, while real doubt and hesitation did not occur, in his practised subject, as often as once in a thousand trials. The admission of doubtful cases he found to be an invitation to the subject to shirk the responsibility of making a decision when the indications of difference were slight; and the exclusion of doubtful cases, so far from making the subject careless, stimulates him to take note of the slight indications which are practically always present. Brown shows more completely than any one before him that even minute changes in the objective difference are reflected in the frequency of the judgments "greater" and "less." His results show very forcibly that there is no warrant for the "threshold of difference," in the old and genuine sense in which it was supposed to be a fact of experience. In the more modern usage of Müller and Titchener, the threshold, or unit of sensation, has become a sort of *Ding an sich*, never appearing in experience, but remaining as an ideal center about which the variations of actual perception are clustered. No one has more heartily rejected such constructions than Professor Cattell.

As the method of right and wrong cases enables us to measure the perceptibility of even very small differences, so the method of discrimination time, introduced for psychophysical purposes by Cattell,[15] and applied most extensively by Henmon,[16] makes it possible to measure the perceptibility of differences that are so large as to be almost beyond the range of error. Taken together, the two methods prove the perception of a difference to be a continuous function of the amount of difference, all the way from zero to quite large values.

---

[14] "The Judgment of Difference," University of California Publications in Psychology, Vol. I., No. 1, 1910.

[15] *Philos. Studien,* 1887, **4**, 250; *Mind,* 1887, **12**, 74; *Philos. Studien,* 1902, **19**, 63.

[16] "The Time of Perception as a Measure of Differences in Sensations," 1906.

Of the results of Professor Cattell's psychophysical studies, that of most general interest is perhaps his proposal to substitute a square root law in place of Weber's law. He was led to the square root law partly by the theory of errors of observation, and partly by the results of experiment. He says:[17]

"The last application of the theory of probability which I wish to make concerns the relation of the error of observation to the magnitude of the stimulus. The algebraic sum of a number of variable errors tends to increase as the square root of the number. In measuring the base line of a survey the variable error of observation increases as the square root of the length of the line. It seems to me the same relation might be expected to hold in a general way when the length of a line is estimated by the eye or compared with another line. Or to take another example, if we estimate one second of time and repeat the trial four times, the algebraic sum of the four variable errors, or the combined error in estimating the four seconds, will tend to be twice as great as the error in estimating a single second. If we estimate or compare the four seconds continuously, the same elements would to a considerable extent be present, and we might expect an error twice as great as in estimating a single second—not four times as great as required by Weber's law. . . . The relation between the error of observation and the magnitude of the stimulus will differ for each stimulus and for each observer, and will not remain constant even for the same stimulus and the same observer. But the usual increase of the error of observation with the magnitude of the stimulus is accounted for in a satisfactory manner by the summation of errors, and I should substitute for Weber's law the following: *The error of observation tends to increase as the square root of the magnitude, the increase being subject to variation whose amount and cause must be determined for each special case.*

"It may be asked if this view is correct, why do the results of researches confirm Weber's law? As a matter of fact Weber's law has not yet been confirmed exactly by any careful research, the error of observation usually becoming larger as the magnitude of the stimulus is taken larger, but almost always more slowly than in direct proportion to the magnitude."

[17] *Amer. Journ. of Psychol.*, 1893, **5**.

In the experiments of Fullerton and Cattell, the variable error increased nearly as demanded by Weber's law, in the case of time of movement, and nearly as demanded by the square root law, in the case of extent of movement, while it fell midway between the two formulæ in the case of force of movement. Some years ago,[18] I collated the results of other experimenters within the field of perception of movement, and found the same general range of results. In nearly every case, the error increases more slowly than demanded by Weber's law, and more rapidly than demanded by the square root. I believe this to be the fact also in other fields, so that Weber's law and the square root law appear as the boundaries of the range of empirical fact. Following the lead of some suggestions made by Fullerton and Cattell, and also incited by the interesting fact that Solomons[19] had attempted a derivation of Weber's law from the combination of variations, and therefore, curiously enough, from essentially the same considerations as those from which Cattell derived the square root law, I have come to the conclusion that the actual increase in the error with increase in the stimulus can be regarded as an index of the degree of correlation between the partial processes included in a total act of perception. I may be permitted to explain this conception a little more fully here.

If we combine two variables in such a way that the variations of the one are completely dependent upon the variations of the other, then for every variation of the one there will be an exactly corresponding variation of the other, and the total variation of the combination will be the arithmetical sum of these two corresponding variations. The variability of the combination will therefore be equal to the sum of the variabilities of the separate variables. Two variables equal to each other in average magnitude and in variability, when thus combined, will give a compound having twice the variability of either of the components; and if a magnitude is composed of several equal variables, all perfectly correlated, the variability of the compound will be proportional to the magnitude—a result corresponding exactly to Weber's law. If, on the other hand, there is no dependence between the variations of the component variables, so that they combine by chance, then the variability will increase as the square root of the magnitude. If there should be a negative correlation between the component variables, the total variability would increase even more slowly than the square root of the magnitude, and if there were a moderate degree of positive correlation, the total variability would increase more rapidly than the square root but less rapidly than the magnitude itself. The lower the correlation between the components, the more nearly would the increase of the total variability approximate to the square root formula, and the higher the correlation, the closer the approximation to Weber's law.

---

[18] "Le mouvement," 1903, p. 194.

[19] *Psychol. Review*, 1900, 7, 234.

A formula now well-known, though, as I suppose, of recent introduction, expresses the general relation between the variabilities of two components and the variability of the compound. If v_1 and v_2 are the variabilities of the components and v_s that of the compound, and if r be the coefficient of correlation between the two components, then

$$v_s^2 = v_1^2 + v_2^2 + 2rv_1v_2.$$

This formula reduces to Weber's law if $r = +1$, and to the square root law if $r = 0$; and I suggest that it be regarded as the general law of combination of variabilities or of errors of observation, and that Weber's law and Cattell's law be regarded as special cases under it, and practically as the limiting cases. If the components are equal and of equal variability, then, calling the variability of each component v_a and that of the compound v_{2a}, we reduce the above formula to the following:

$$v^2_{2a} = 2v_a^2(1 + r),$$

or,

$$v_{2a}/v_a = \sqrt{2(1 + r)}.$$

Doubling the magnitude, then, increases the variability in the ratio given by this equation. If we know r, we can determine the increase of variability, or if we measure experimentally the increase of variability, we can calculate r from this equation,[20] and express the result of our experiment finally as a certain value of r.

For example, if we assume for the moment that it is legitimate to apply this simple analysis to the process of reproducing movements, we may take the values for the variable error, obtained in experiments by Fullerton and Cattell,[21] and compute r, as follows.

---

[20] If, instead of compounding two variables, we compound any number, n, equations analogous to the above can be obtained, leading finally to the following, in which v_a is the variability of each component, v_{na} that of the compound, and Av r the average of all the coefficients of correlation between the components taken two by two:

$$v_{na}/v_a = \sqrt{n + n(n - 1)\text{Av } r},$$

from which, given the ratio of increase of the variability, we can compute the average correlation existing among the components.

[21] P. 48.

In reproducing an arm movement of 100 millimeters, the variable error was 5.3; and in reproducing 500 millimeters, the V.E. was 9.5. The increase is here even slower than called for by the square root formula, and the correlation comes out as $-.09$.

In some experiments on the force of movement,[22] the V.E. was 12.3 for a standard of 2 kilograms, 20.5 for a standard of 4 kilograms, and 37.1 for a standard of 8 kilograms. As between 2 and 4 kilograms, r here $= +.40$, as between 4 and 8, $r = +.64$, and as between 2 and 8, $r = +.43$.

In some experiments on the time of movement,[23] the V.E. was 27.9 for a standard of 1/4 second, 52.0 for a standard of 1/2 second, and 99.8 for a standard of 1 second. Here, as between 1/4 and 1/2 second, $r = +.73$; and between 1/2 and 1 second, $r = +.84$; and as between 1/4 and 1 second, $r = +.75$.

In some of Fechner's experiments[24] on lifted weights, the error increased in the ratio of 1 to 1.66 in passing from a standard of 1,000 grams to one of 2,000 grams, giving $r = +.23$; and increased in the ratio of 1 to 1.74 in passing from 1,500 to 3,000 grams, giving $r = +.57$.

It remains to consider whether the conception of combination of errors or variations can be justly applied to the perception of magnitudes. Can the perception of a large magnitude be conceived as a sum or combination of the perceptions of smaller magnitudes? Since we are here concerned with the total physiological processes involved, and not simply with the conscious sensation, we are not entangled in the old difficulty of considering a large sensation as a sum of smaller sensations. The reaction of the organism to a stimulus is always a complex activity, composed of many elementary activities; and the reaction to a greater stimulus is undoubtedly a larger complex, composed of more numerous elementary activities, than the reaction to a smaller stimulus. Each elementary activity is variable, and these elementary variations combine in the process leading to the final reaction or percept. Hence there can be no doubt that, in a broad way, the law of combination of variations applies to the process of perception. In the case of extent, or of time, as shown by Fullerton and Cattell, the perception of a large magnitude can rather easily be thought of as a combination of perceptions of smaller magnitudes, though with a degree of fusion or continuity in the total that might easily modify the outcome. A stimulus of two inches, whether presented to the eye or to the skin, enters the system by way of twice as many elementary end-organs as does a stimulus of one inch. Each of these end-organs is variable in its activity, and the variations must combine in the further course of the reaction. In the case of intensive magnitudes, the case is not so clear because we do not know that the more intense stimulus excites more numerous elementary end-organs than the less intense; and, in fact, we do not know in what the difference of intensity consists within the sense organs. We do know, however, that the more intense stimulus gives rise to a wider diffusion of nerve currents, and this fact gives reason to believe that the number of nerve elements engaged in the perception increases with increase in the intensity of the stimulus. If so, the law of combination of variations would be at least partially applicable to the perception of intensive magnitudes.

---

[22] P. 84.

[23] P. 107.

[24] *Psychophysik*, 1889, I., 193.

There is much more in the psychophysical work of Professor Cattell that is worthy of mention. I might mention the new apparatus devised; the experiments with lights presented successively, showing that vision, deprived of its advantage of simultaneous presentation, loses its superiority to the other senses in fineness of discrimination. I might rehearse the evidence against the Müller-Schumann theory of the perception of lifted weights—evidence which has never been deprived of its force; and I might review the later work on movement done in Cattell's laboratory, as that by Hollingworth.[25] I will, however, pass these matters by, and only allude, in closing, to one general aspect of Professor Cattell's work. I have spoken of his predilection for quantitative experiments. Along with this has gone a keenly critical attitude towards quantitative results, and an insistence that results must be sufficiently uniform and reliable to warrant the conclusions. This critical attitude is in striking contrast to the procedure of many psychologists, and even of some great psychologists, who have put forward conclusions based on meager data, without duly considering the variability that appears in all mental performances and the wide range of possible error. A considerable share of the published work in quantitative psychology has been of little real value, or at the best suggestive rather than conclusive, because of the neglect of the probable error. All pupils of Professor Cattell will bear witness of his attention to this matter of the probable error. How often have we presented our experimental results in seminar, and drawn conclusions which seemed to us to be indicated, only to have our attention called to the probable error of our averages, and to be sent back to our experiments to gather more data, or more accurate data! The experience was often disheartening at the time, but the outcome has been a high standard of reliability in the work issuing from the laboratory. If there has been in recent years a general improvement in this respect throughout the field of psychological investigation, I can not doubt that it is traceable, in part, to the influence of Professor Cattell, exerted through his writings and through his pupils, some of whom have taken an active part in the progress of quantitative psychology.

---

[25] "The Inaccuracy of Movement," 1909.

R. S. Woodworth

PROFESSOR CATTELL'S STUDIES BY THE METHOD OF RELATIVE POSITION

(THE ORDER OF MERIT METHOD)

In the physical and exact sciences, in which the phenomena to be measured are usually in themselves quantitative in character, measurement is facilitated by the development of standardized spatial and temporal scales. When the facts to be measured are in themselves spatial and temporal changes, these scales may be applied directly and quantitative treatment is at once possible. In other cases the phenomena may be indirectly measured by means of correlated spatial and temporal changes.

In economic science, and in the processes of exchange, where the phenomena to be measured are not spatial or temporal facts, but are rather of the nature of subjective values, measurement and quantitative treatment are made possible by the development of a medium of exchange, to the units of which are attached values which parallel those of the facts to be measured.

In the biological and social sciences, when quantitative treatment is at all possible, it is either of spatial or temporal changes directly or is in terms of the frequency of occurrence of qualitative facts which are in themselves determinable and identifiable.

The psychologist finds it necessary to deal with phenomena which are neither directly nor indirectly spatial or temporal, but are essentially of an intensive or qualitative character, and which can not be easily related to objective scales. In this science measurement first began in the case of those experiences which have as their immediate cause or correlate external series of facts either in themselves immediately spatial or temporal (as in subjective estimates of duration, or of linear magnitude), or series of facts which could be themselves measured by the indirect use of objective standards (as in judgments of weights, lights, sounds, etc.) The quantitative treatment of these psychological processes led to that branch of experimental psychology which is known as psychophysics. Closely related to psychophysics are the psychometric measurements which are interested in the temporal relations of processes which are in other respects not measured.

Of much greater difficulty is the measurement of psychological facts which are neither in themselves immediately quantitative, nor yet directly related to phenomena which can be quantitatively described. Affective experiences such as pleasantness, preference, interest; more complex facts such as feelings of similarity and difference, confidence, belief, conviction; social or personal evaluations such as eminence, usefulness, character; literary and artistic facts such as beauty, eloquence, style; educational facts such as legibility of penmanship, ability in drawing, excellence in composition; or traits which have high industrial or commercial importance such as skill, leadership, zeal, persuasiveness; all these are experiences or traits for which it is highly desirable to develop precise methods of measurement, analysis and description. Such facts as these are distinctly psychological, either in that they are known only to the immediate experience of an individual and show large dependence on individual differences (as preference, beauty, etc.) or in that their quality arises directly out of their effect on the consciousness and behavior of other individuals, this effect not being susceptible of spatial and temporal measurement. They are, in other words, subjective facts, for the measurement of which objective scales either can not or do not exist.

There is also another type of subjective fact represented by graded series of stimuli in which the steps are so small that different individuals' judgments or the same individual's judgments on different occasions differ measurably with respect to the position to be assigned a given item in the graded scale. In this case there may or may not exist quantitative methods of determining the objective relations of the different items, but in any case the diverse judgments do not always agree with the known relations. Psychologically the items take their place in a qualitative series, and in an order which varies with the occasion of the determination.

An early contribution of Professor Cattell, and one which has proven of unusual value to psychological procedure, presented a method of converting such a qualitative series into a series of quantitative differences. It is obvious that a method of treating facts which are subjective chiefly because of their imperceptible differences will be serviceable also in the measurement of facts which are subjective for widely different reasons.

Thus "if one endeavors to arrange and rearrange in serial order a number of given objects, the positions successively given them will vary somewhat as they would vary if the arrangements had been made one each by different observers. If we undertook to arrange ten times a series of grays in order of brightness, we should no more get the same order each time than we should get identical orders from ten different subjects. . . . The judgments of the same individual at different times are theoretically quite comparable to those of different individuals regardless of the factor of time.

"In this way may be illustrated a continuum between the subjective and objective classes of judgment. In the cases of grays, weights or lines, we assume a certain standard which we term the objective order, and which we determine through photometry or some analogous method. Because we have such methods we do not need to have recourse to individual judgments to determine objective values, and these individual judgments give us a part of the personal equation . . . . On the other hand we have such subjective judgments as preferences in sculpture, painting or music. In the first class we may arrange individuals in precise order for accuracy of discrimination; in the second, one may with equally good taste vary his preferences within a considerable range. So far as any distinction on a statistical basis is possible, we might consider as subjective those types in which the various judgments of the individual formed a species of their own, varying from each other considerably less than from an equal number of judgments made by different individuals; and consider as objective those in which an individual would vary from his own independent judgments about as much as the variation of an equal number of judgments by different observers. . . . The two categories would almost certainly be continuous."[1]

Since Professor Cattell's original account of his first application of the method of relative position is relatively inaccessible to the general reader, that account is here reproduced. It consists of that portion of the larger article[2] relating to the use of the order of merit method.

"In the experiments on the intensity of light, I used various methods. These included a method of securing a given physical difference by altering the angle of incidence of the rays of light, which constitutes, I believe, a new form of photometer, having certain advantages. I found it, however, most convenient to use gray surfaces, reflecting a known percentage of the light. These surfaces were washed with Indian ink. The ink was made as black as possible, and one piece of drawing paper was washed; then a drop of water was added to the ink, another piece was washed, and the process was continued until the wash became imperceptible. In this way 211 shades between black and white were obtained.

---

[1] Wells—"On the Variability of Individual Judgments," p. 511.

[2] "The Time of Perception as a Measure of Differences in Intensity," *Philos. Studien*, 19, 63–68, 1902.

These grays (in cards 5 cm. sq.) were then tested by various photometric methods. The series proved to have a fairly regular gradation, except that the steps increased toward the white end, the gray 95 steps from white being midway between white and absolute black.

"Such a series of grays (a hundred grays forming equal physical steps could be selected that would be preferable to this series of 211 cards) is suitable for testing the accuracy of discrimination and its variation under different circumstances, as with the intensity of stimulus, in memory, etc. The physical differences are smaller than can be perceived, and if an observer arranges the grays as nearly as possible in the order of brightness, errors occur which measure the accuracy of discrimination. Thus in ten trials—the first two by the present writer and the others by eight different observers—the errors of displacement were as follows: 6.63, 7.97, 6.04, 6.2, 7.44, 7.81, 8.29, 9.68, 11.5. The arrangement can be made in about an hour, and, as there are over 200 separate judgments, the average has a probable error of only about 0.5. Observers differ within the extremes of about 1: 2 which agrees with other determinations. For the more accurate observers the error is six cards, or about 1/35 of the range between black and white. The degrees that appeared just noticeable when these were selected were between 13 and 32, but errors occurred in the case of the larger number.

"When the results of the ten series are combined and the relation of the error of displacement to the intensity is considered, it is found that while the error increases with the stimulus, it does so more slowly than required by Weber's law. The average displacement for each of seven groups of 30 cards (allowance being made for the longer steps between the brighter cards) was, approximately, 13.61, 10.6, 12.14, 10.03, 6.46, 5.09, 2.98.

I	II	III	IV	V
85	5.44	1/16	6.4	59
68	4.02	1/16	5.9	49
56	4.85	1/14	8.7	65
44	4.01	1/11	9.1	60
32	2.58	1/12	8.1	46
20	2.03	1/10	10.2	45
8	1.19	1/7	14.9	42

"In the table are given: (I.) the approximate percentage of light reflected from the middle gray in each of the seven groups, that is the magnitude of the stimulus; (II.) the error of observation in terms of percentages of light between white and absolute black; (III.) the error of observation in terms of the magnitude of the stimulus to the nearest whole fraction; (IV.) the error of observation in terms of percentages of the magnitude, and (V.) the error of observation in terms of percentages of the square root of the magnitude.

"It appears, consequently, that as the stimulus increases the error of observation increases, but not so rapidly as the stimulus, being about 1/7 with the weakest and 1/16 with the strongest stimulus. It increases much more nearly in direct proportion to the square root of the stimulus, in accordance with the hypothesis I have proposed in place of Weber's law. This hypothesis—'The error of observation tends to increase as the square root of the magnitude, the increase being subject to variation whose amount and cause must be determined for each special case'—has not yet received the consideration which the experimental evidence and theoretical explanation seem to me to warrant."

In a later reference[3] we find these experiments succinctly summarized in the following words:

"Some two hundred shades of gray were made, giving approximately equal differences in illumination between white and black. In such a series the grays toward the white end appear more alike than those toward the black end, and two adjacent grays are indistinguishable. Psychologically it is a qualitative series. If now the grays are arranged in the order of brightness a number of times by the same or different observers and the average position in the series of each gray is determined, the mean variation is inversely proportional to the psychological differences between the grays. There is thus determined the quantitative difference in the perception and its relation to the physical differences between the lights. The same methods have been used in the Columbia laboratory to measure the validity of beliefs, the beauty of pictures, differences in traits of character, literary skill and efficiency in various performances."

This use of the method of relative position in psychophysics represents its application to the measurement of facts which are subjective chiefly because of their small differences in perception. The judgments are objective, in the sense that their correctness is determinable by comparison with photometric or similar measurements. From this to the application of the method to the measurement of facts not determinable by such objective means, and subjective not only because of the amounts of difference presented, but more especially because of individual differences in the judgments of various observers, was a most natural step. Professor Cattell took this step when in 1906 he published his study of American men of science. (The study was begun in 1902 and a report of the data concerning the group of psychologists was published in 1903.) As characteristic of his point of view in psychology the introductory words to this report are of special interest.

---

[3] "A Statistical Study of American Men of Science," *Science*, N. S., Vol. XXXII., pp. 633–648, 672–688, 1910.

"The psychologist, like the student of other sciences, can view his subject from different standpoints and pursue it by various methods. He may get what knowledge he can of mental processes by introspection, or he may use objective methods. He may confine himself to the 'inner life,' or he may study the individual in all his psychophysical relations. He may give verbal descriptions, or he may make measurements. He may describe static mental life, or he may study the lower animals and human beings from a dynamic and genetic point of view. He may attempt to determine facts and laws that hold for mental life in general, or he may attend to individual differences. He may ignore the practical applications of his science, or he may investigate them. Psychology has until recently concerned itself chiefly with the first of these various alternatives. But its recent progress and future development seem to the present writer to depend particularly on the second. In this case, our two main methods, which can often be combined, are experiment and measurement in the laboratory, and the inductive and statistical study of groups of individuals. In recent years great progress has been made in both directions. Experimental psychology has become a science coordinate with the other great sciences, and statistics have been extended to include sociological and moral phenomena."[4]

As material for intensive studies of groups of individuals, and the nature and origin of their differences, three groups of people were selected. They were, (*a*) a thousand students of Columbia University, selected on the basis of their willingness to submit themselves, upon entrance and graduation, to a series of mental and physical tests; (*b*) the thousand most eminent men and women in history, selected on the basis of the amount of space given them in several standard biographical dictionaries and encyclopedias; (*c*) a thousand American men of science, selected on the basis of their ranking, by the method of relative position, in their respective sciences, at the hands of ten leading workers in each field. It is the study of this third group which is of interest in the present connection. It may be well to give a general account of this application of the order of merit method, in the form of selected quotations from Professor Cattell's own accounts of it.

---

[4] *Science*, N. S., 24, 658, November 23, 1906.

"Many of the problems that the writer had in view in the present research might be solved by the study of any group of a thousand American men of science, so long as they had been objectively treated. The objective selection of a group sufficiently large for statistical treatment is however essential. As cases can be quoted to illustrate the cure of nearly every disease by almost any medicine, so examples can be given in support of any psychological or sociological theory. The method of anecdote, as used by Lombroso, may be readable literature, but it is not science. A thousand names might have been selected by lot from all the scientific men of the country, assuming a list to have been available, but a group of the thousand leading men of science arranged in the order of merit has certain advantages. Information in regard to them can be better obtained than in the case of those who are more obscure. Correlations can be determined between degrees of scientific merit and various conditions. The comparison with a similar group selected ten or twenty years hence, or with a similar group of British, French or German men of science, would give interesting results. The list itself, if printed after an interval of twenty years, would be a historical document of value. Lastly, the data can be so used as to carry quantitative methods a little way into a region that has hitherto been outside the range of exact science. It is this last problem which I wish to take up in this paper."[5] . . .

"In selecting a group of a thousand scientific men, the number in each science was taken roughly proportional to the total number of investigators in that science . . . . The individuals were selected by asking ten leading representatives of each science to arrange the students of that science in the order of merit. There were for each science slips made with the names and addresses of all those known to have carried on research work of any consequence. The total number assigned a position was 2,481, distributed among the sciences as follows: Mathematics, 201; physics, 261; chemistry, 389; astronomy, 165; geology, 257; botany, 213; zoology, 290; physiology, 101; anatomy, 89; pathology, 251; anthropology, 72; psychology, 192. These numbers included duplications when a man was given a place in more than one science. . . .

"The ten positions assigned to each man were averaged, and the average deviations of the judgments were calculated. This gave the most probable order of merit for the students of each science, together with data for the probable error of position of each individual. The students of the different sciences were then combined in one list by interpolation, the probable errors being adjusted accordingly. The list contains 1,443 names, of whom the first thousand are the material used in this research."[6]

---

[5] "American Men of Science"—Second Edition, 1910, p. 544.

"The average of ten judgments is not necessarily more correct than any one of these judgments; the conditions are similar to observations in the exact sciences. One good observation may have more validity than the average of a number of observations made under less favorable conditions. But if ten scientific men concerning whose competence it is not possible to discriminate in advance make a judgment, we may take the average as the most probable value. If we had but a single judgment we should not know its validity, but with ten judgments the probable error can be calculated. These probable errors tell us not only the limits within which the place of an individual in the series is likely to be correct, but also measures the differences between the individuals."

"The method enables us to measure not only differences in scientific merit, but also the accuracy of judgment of those who make the arrangements. It would be possible to determine whether those more eminent would have the more accurate judgments, at what age the individuals are most competent and the like. As a matter of fact, the judgments in the present case were made by those most eminent in each science who were willing to undertake the task."[7]

The primary results of this study may be illustrated (*a* by the) data concerning the distribution of the leading thousand men of science; (*b*) by the account of the intensive study of the members of one of the scientific groups, the psychologists; and (*c*) by the comparison of the first arrangement with a second similar arrangement, made after an interval of several years.

(*a*) "In the accompanying curve . . . is shown the distribution of the thousand men of science. The 1,000 scientific men are divided into ten groups, the range of eminence or merit covered by each hundred being proportional to the space it occupies on the axis of the abscissas, and the number of each degree of ability being proportional to the ordinates. The range of merit covered by each hundred becomes smaller and there are more of each degree of merit as we pass from the first to the second hundred and so on for the first five hundred, after which the differences become very small. The first hundred men of science cover a range of merit about equal to that of the second and third hundreds together, and this again is very nearly equal to the range covered by the remaining seven hundred. The average differences between the men in the first hundred are about twice as great as between the men in the second and third hundreds, and about seven times as great as between the men in the remaining groups. Or the differences among the first hundred are almost exactly ten times as great as among the last five hundred, who differ but little among themselves. It would be desirable to compare this distribution with that of the probability integral and with the salaries paid to scientific men, but the data are not as yet at hand." [8]

---

[6] *Ibid.*, 538 ff.

[7] *Ibid*, 566.

(*b*) The study of the group of the fifty leading psychologists yields many results of immediate interest. It also illustrates the

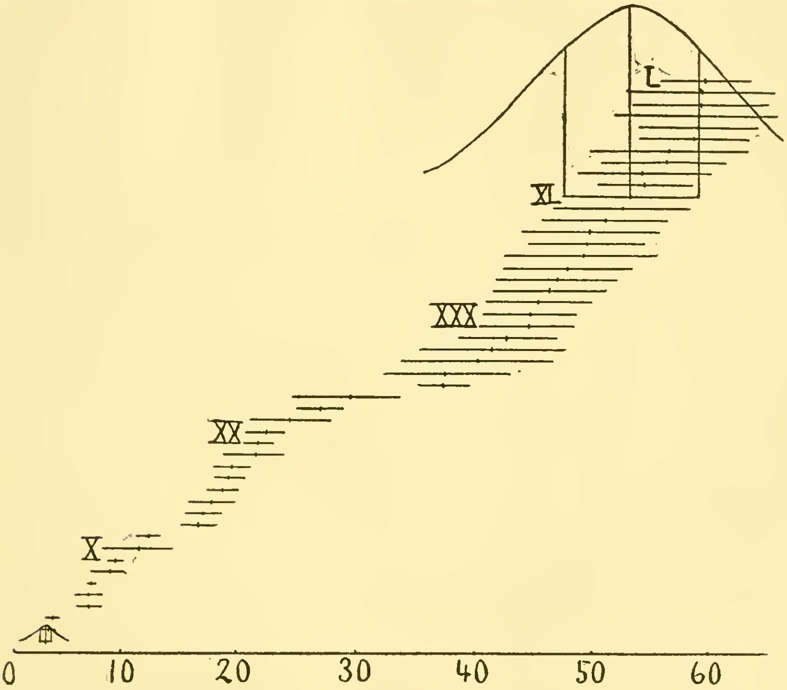

FIG. 1. The positions and probable errors of the fifty psychologists.

attempt to secure quantitative treatment, statistical analysis and mathematical exactness, so characteristic of all of Professor Cattell's work and teaching.

"In order to illustrate further the serial distribution and the probable errors, I have made a diagram for the fifty psychologists. The grade of each, no judgments being omitted, is shown by the vertical mark, and the length of the line indicates the probable error or range within which the chances are even that the true position falls. Thus the psychologist who stands first on the list was . . . given this position by the independent judgment of all. The psychologist who stands second has, as shown on the diagram, a position of 3.7 and a probable error of 0.5, *i. e.*, the position of 3.7 is the most probable, but the true position is equally likely to be within the short horizontal line, between 3.3 and 4.2, or outside it. It must, however, be remembered that the chances of the true position being far outside the range of this line decrease very rapidly. Over it is roughly drawn the bell-shaped curve of the normal probability integral. The true position is along the base line covered by this curve, and the chances of its being at any given point are proportional to the ordinate or height of the curve above the base line. There is only one chance in about six that the true grade is above 2.7 or below 4.7, and only one chance in about 150 that the true grade is above 1.7 or below 5.7. It will be seen from the diagram that while the positions of the psychologists II., III. and IV. are the most probable, the relative order is not determined with certainty. On the other hand the chances are some 10,000 to one that each of these psychologists stands below I. and above V."

---

[8] *Ibid.*, 552.

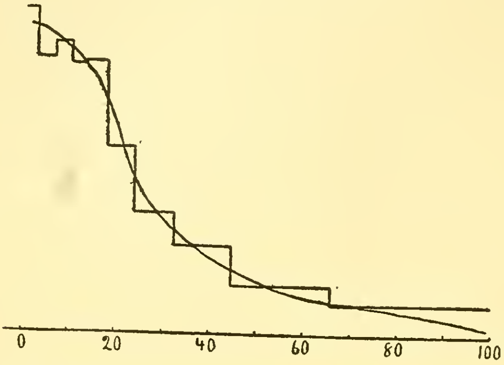

FIG. 2. Distribution of the thousand men of science.

"It is evident that the probable errors increase in size as we go down the list. The curve of distribution drawn over No. XL. indicates that the chances are even that the true position falls between the grades of XXXIV. and L. and that there is one chance in four that he does not belong among our fifty leading psychologists.

The increase in the size of the probable errors is irregular, it being more difficult to assign a position to some men than to others."

"It will be noted that the psychologists fall into groups, the first twenty being set off from the next group, though the two groups are bridged over by three cases. At this point also the probable errors become almost suddenly about three times as large. There are altogether about 200 psychologists in the country, and it looks as if the first tenth forms a separate group of leaders. There is a similar, though less marked group of the first twenty astronomers, but these groups seem to be partly accidental. There is, however, as shown below, an inflection point in the curve of distribution after about the first tenth of our scientific men. The first twenty psychologists fall into four distinct groups, and there are groupings in the other sciences. They do not, however, appear to be sufficiently marked to lead us to distinguish species, such as men of genius and men of talent. It is, however, possible that the complicated conditions may ultimately be analyzed so as to give such groups."

"The probable errors not only tell the accuracy with which the psychologists can be arranged in the order of merit, but they also measure the differences between them. This, indeed, I regard as the most important result of this paper, as science is advanced chiefly by the extension of quantitative methods, and it might not have been foreseen that it would be possible to measure degrees of scientific merit. Our data are concerned with the recognition of scientific performance, not with abstract ability, if such a thing is conceivable. Merit is in performance, not in non-performance, and expert judgment is the best, and in the last resort the only, criterion of performance."

"The difference in scientific merit between any two of the psychologists whose positions and probable errors are shown in the chart is directly as the distance between them and inversely as their probable errors. If two of them are close together on the scale, and if the probable errors are large, the difference between them is small, and conversely."

"If the psychologists II. and III. were separated by 0.5 and their probable errors were 0.5, as is approximately the case, then the difference between them is so small that there is one chance in four that the position of III. is above the grade of II. If again the psychologists XL. and XLIX. were separated by 6 and their probable errors were 6, as is again approximately the case, then there is again one chance in four that the true position of XLIX.

is above the grade of XL. The difference between II. and III. is thus about the same as that between XL. and XLIX."

"If we take the fifty psychologists in groups of 10, and thus partly eliminate the chance variations, the average probable errors of the five groups are 0.7, 1.8, 4.2, 5.8, 6.2. These probable errors are subject to a correction for the range covered by the grades. Thus the first ten cover a range of about eleven points, and the last ten a range of about six points, and the differences between the psychologists at the top of the list would be nearly twice as great as between those at the bottom of the list if the probable errors were the same. When we take account of both factors, the probable errors in the five groups are 0.6, 1.9, 1.8, 6.4, and 10.7. While the probable errors are determined with a considerable degree of exactness, which is itself measured, the ranges covered by the grades seem to depend on the special conditions in the science; they are not the same in the different sciences, and their validity cannot be determined with any exactness. Subject, however, to a considerable probable error, the range of merit covered by the fifty psychologists is inversely as the figures given, and reduced to a scale of 100 would be: 55.6, 17.5, 18.5, 5.2 and 3.2."

"Thus we can say that the psychologists at the top of the list are likely to differ from each other about 18 times as much as the psychologists at the bottom of the list. We have no zero point from which we can measure psychological merit. Men who are 6 ft. 2 in. tall are likely to differ from each other about ten times as much as men who are about 5 ft. 8 in. tall, though the difference in their height is only as 68 : 74. Even though we assumed the zero point to be where psychological performance begins or at the survival minimum of human ability, we should only obtain relative differences."[9]

(*c*) About seven years after the selection of the group of scientific men "it seemed desirable to repeat the process of determining the thousand leading scientific men in the United States. It is worth while to learn what changes have taken place in the composition of this group and in the distribution of the scientific men among various institutions and in different parts of the country. A list of scientific men as nearly contemporary as might be was also wanted for some further studies of the conditions of heredity and environment which are favorable to scientific productivity. The methods used . . . were substantially the same as before and need not be redescribed in detail."[10]

---

[9] *Ibid.*, 546–7.

[10] *Ibid.*, 565.

On the basis of the two arrangements seven years apart, and with the aid of additional information as to the distribution of the men, etc., an attempt was made to throw light on a great many questions of importance to science in general, and of particular interest to the science of education. The changes in position during the seven year period; the correlation between age and advancement; the distribution of scientific men in various institutions and localities; the relative strength of the various departments and scientific faculties of different institutions; the productivity of scientific men in different sections of the country and in different educational centers; the correlation between eminence and the age of receiving the various degrees; the comparison of the conditions of scientific success in the different sciences; these and other equally interesting questions received such illumination that it is quite impossible to summarize the results here. The influence of these studies of Professor Cattell on the general interests of education in America can not be easily estimated, and all of the original reports merit careful and detailed study by both students and administrators. Of the various contributions which the studies have made, the purely methodological portions are not the least important.

The application of the order of merit method was of course but incidental to the larger purposes of these studies. The general motive and point of view are well expressed in the following characteristic words.

"It is surely time for scientific men to apply scientific methods to determine the circumstances that promote or hinder the advancement of science. We should begin where and when we can; even though the results of the first efforts may appear somewhat trivial, we may proceed in the confident belief that in the end the advancement of science will become an applied science."[11]

"It is not altogether without interest to find that it is possible to reduce to order facts which might be supposed to be outside the range of the natural and exact sciences. The present articles are, however, only a beginning of a study of scientific men as a group and of the conditions on which scientific performance depends. We have in a large measure explored the material world and subdued it to our uses; it is now our business to secure an equal increase in our knowledge of human nature and to apply it for our welfare. If he is a benefactor to mankind who makes two blades of grass grow where one grew before, his services would be immeasurably

[11] *Ibid.*, 565.

greater who could enable two men of science to flourish where there had been but one."[12]

The method of measurement by relative position in a series has been adopted by a number of Professor Cattell's students and colleagues, as well as by several other workers, in the investigation of facts to which objective scales can not be easily applied. These various studies may be roughly grouped under the headings individual differences, education, esthetics, judgment, commerce, and methodology.[13]

In the field of individual differences, Sumner has studied age, sex, and class differences in the ability to arrange one's beliefs along a scale of certainty. Norsworthy has studied personal estimates of individual differences in ability and character, and the conditions and variability of these estimates. Kuper has used the method in investigating the interests of children and the dependence of these interests on such factors as age, sex and nationality. Downey has employed the method in her study of family resemblance in handwriting and the degree to which such resemblance is due to heredity. On the basis of the same method Fernald has devised an ethical discrimination test which he has used in the examination of defective delinquents.

Applications of the method in education are represented by Thorndike's scales for the measurement of the legibility of penmanship and achievement in drawing and by the scale devised by Hillegas for the measurement of excellence in English composition. Yerkes has used the method as a suggestive method of instruction and of class exercise in psychology.

In esthetics Wells has used the method in his statistical study of literary merit, with special reference to the possession of such qualities as charm, clearness, originality, euphony, wholesomeness, etc. The same investigator has used picture postal cards as stimuli in a study of the variability of judgments of preference and beauty. The present writer has used the method in a study of the types and tendencies of comic situations and Downey has employed it in her study of the imaginal reaction to poetry and the variability of affective judgments.

In commerce the method was given an interesting and useful application by E. K. Strong, Jr., and the present writer, when it was found that the laboratory study of advertisements, sales letters, etc., affords significant measurements of "pulling power" which correlate closely with the actual value of the various appeals as determined by the number of replies or inquiries, the cost per inquiry, the number of sales, etc. This and related work led to the establishment, by a group of practical business men, of a research fellowship in applied psychology in Columbia University, which has now been continued for three successive years.

---

[12] *Ibid.*, 563.

[13] At the end of this paper is given a bibliography of the method of relative position, which is complete to the date of publication.

In the field of judgment, Wells, Downey and the writer have made numerous studies by the method of relative position, in which, aside from the measurements and their usefulness in general and applied psychology, a number of problems concerning the judgments themselves have arisen. Among these problems may be mentioned as typical examples, the variability of judgment in different parts of the series, the certainty of individual preferences and aversions, group differences in likes and dislikes, individual differences in consistency of judgment and in judicial capacity, the correlation between these two characteristics, consistency and judicial capacity in different situations, the relation of variability to the length of the series, the influence of the form of expression on the outcome of judgments, and the more general problem of securing quantitative criteria of the degree of subjectivity possessed by various types of judgments.

From the purely methodological point of view Barrett has made an empirical comparison of the relative advantages of the order of merit method and the method of paired comparisons, demonstrating the numerous technical superiorities of the former method. The present writer has proposed a modification of the strict order method, designated the *group method.* This method, in addition to being simpler and more easily carried out, correlates closely with the results from the same observers by the strict order method and gives opportunity to observe changes in the evaluation of the series as a whole. Thorndike has discussed certain of the statistical features of the method of measurement by relative position, and the possibility of transmuting such a qualitative series into a quantitative distribution on the basis of the curve of the probability integral.

Several studies are now in progress in which the method of measurement by relative position is being used in the study of such topics as belief, judgment, eminence, affective qualities, special abilities, individual differences, and a number of specific problems in applied psychology.

The method has proven to be a real contribution to the technique of experimental, statistical and applied psychology. It is especially useful in that type of psychological investigation which is more interested in outcome than in content; in the way in which mind works, rather than in what is in the mind at the moment of its operation.

H. L. Hollingworth
Columbia University

BIBLIOGRAPHY ON MEASUREMENT BY RELATIVE POSITION (THE ORDER OF MERIT METHOD)

Barrett, Mabel, The Order of Merit Method and the Method of Paired Comparisons, *Jour. Phil., Psych., and Sci. Meth.*, July 3, 1913, 382–4.

Cattell, James McKeen, The Time of Perception as a Measure of Differences in Intensity, *Philos. Studien*, 19, 63–68, 1902.

A Statistical Study of Eminent Men, *Pop. Sci. Mo.*, 53, 357, 1903.

Statistics of American Psychologists, *Amer. Jour. Psychol.*, 14, 310–328, 1903.

A Statistical Study of American Men of Science, *Science*, N. S., XXIV, No. 621, 658–665, Nov. 23, 1906; No. 622, 699–707, Nov. 30, 1906; No. 623, 732–742, Dec. 7, 1906.

A Further Statistical Study of American Men of Science, *Science*, N. S., XXXII, No. 827, 633–648, Nov. 4, 1910; No. 828, 672–688, Nov. 11, 1910.

American Men of Science. (In the appendix of this volume the two foregoing studies are reprinted, in the first edition (1906) the first study, and in the second edition (1910) both the first and the second studies.)

Downey, June E., A Preliminary Study of Family Resemblance in Handwriting, Bull. No. 1, Dept. Psychol., Univ. of Wyoming, 1910.

The Imaginal Reaction to Poetry,—Bull. No. 2, Dept. of Psychol., Univ. of Wyoming, 1912.

Fernald, G. E., The Defective Delinquent Class, Differentiating Tests, *Amer. Jour. of Insan.*, 69, 125–142, 1912.

Hillegas, Milo B., A Scale for the Measurement of Quality in English Composition by Young People.—*Teachers College Record*, XIII, 4, Sept. 1912.

Hollingworth, H. L., Judgments of the Comic, *Psych. Rev.*, 18, 132, 1911.

Judgments of Persuasiveness, *Psych. Rev.*, 18, 234, 1911.

The Influence of Form and Category on the Outcome of Judgment (With Margaret Hart Strong), *Jour. Phil.*, 9, 513, 1912.

Advertising and Selling, New York, Appletons, 1913.

Experimental Studies in Judgment, Archives of Psychology, No. 29, 1913.

Kuper, Gertrude M., Group Differences in the Interests of Children, *Jour. Phil.*, July 4, 1912, 376–379.

Norsworthy, Naomi, Validity of Judgments of Character, Essays in Honor of William James, 1908.

Strong, E. K., Jr., The Relative Merits of Advertisements, Archives of Psychology, No. 17, 1911.

The Relative Merits of Advertisements, *Advertising and Selling*, 1911.

Application of the "Order of Merit Method" to Advertising, *Jour. Phil.*, Oct. 26, 1911, 600–606.

Psychological Methods as Applied to Advertising, *Jour. of Educational Psychol.*, Sept. 1913, 393–404.

Sumner, F. B., A Statistical Study of Belief, *Psych. Rev.*, 5, 616–631, 1898.
Thorndike, E. L., Handwriting,
  Mental and Social Measurements, 2d ed., 1913.
  The Measurement of Achievement in Drawing. *Teachers College Record*, XIV, 5, Nov. 1913.
Wells, F. L., A Statistical Study of Literary Merit, ARCH. PSYCHOL., No. 7, 1907.
  On the Variability of Individual Judgments, Essays in Honor of William James, 511, 1908.
Yerkes, R. M., Introduction to Psychology, Ch. XIV., 1911.

PROFESSOR CATTELL'S RELATION TO THE STUDY OF INDIVIDUAL DIFFERENCES

THE differences in intellect, character and skill which separate individual human beings from the average or from the modal condition of man as a species seem to us now as obvious as the differences between species of animals, and as important for the understanding and control of nature as the common features of human thought and action. Twenty-five years ago, however, the majority of psychologists neglected their existence and substantially denied their importance. Neither the English psychological tradition as codified by Bain, nor the newer experimental work of which Wundt won the leadership, showed any clear promise of enriching mental science by studies of the variations of individual men in response to the same external situation. Except for certain stock references to temperament, imagery, or the like, diversities were hidden in averages, or even discarded as erroneous.

Against this narrowness Professor Cattell was perhaps the first rebel from within the ranks of psychologists, Galton being the prime mover from without. In his first published paper (of twenty-nine years ago)[1] we find him printing full individual tables, reporting separately the curves for two divergent individuals, and emphasizing the fact that no measure taken had been omitted from his report. Shortly afterward, we find him actively promulgating the work of Galton. Galton had been quick to appreciate Quetelet's idea of describing individual differences quantitatively and put it to use over a wider field. Cattell refined Galton's methods and won recognition for such measurement of individuals as a standard division of psychology and of psychological training in universities, beginning at Pennsylvania the systematic inventory of mental traits which became so important a feature of the Columbia laboratory and which was for so many of us an introduction to the whole topic of individual psychology. His paper of 1890 on "Mental Tests and Measurements"[2] was the first of a series of influential contributions made during that decade and associated primarily with the names of Kraepelin, Binet, Cattell and Jastrow.

Individual psychology, diagnostic tests and measurements of efficiency are now the fashion. What Galton cried of in the wilderness and Cattell took pains to prove was a possibility is now codified in text-books like Whipple's, a familiar feature of school surveys and children's courts, sought after by bureaus of vocational guidance. Binet seems likely to become a household word amongst teachers as Babcock is amongst dairymen. Stern, in 1911, listed over fifteen hundred titles under Individual Psychology, of which at least five hundred represent actual quantitative work. More papers are now read upon Mental Tests and allied topics at a meeting of the Psychological Association than upon all the results of introspective analysis together.

---

[1] "Ueber die Zeit der Erkennung und Benennung von Schriftzeichen, Bildern und Farben," *Phil. Studien*, Vol. II., pp. 635–650.

[2] *Mind*, Vol. 15, pp. 373–380.

This extraordinary extension of individual psychology represents very great advances as well; and the value of the early work by Cattell, Jastrow, Binet and Kraepelin is perhaps best shown by its power to provoke further work, both by them and by others, which criticized and amended it. Of this I may speak later. I wish first to note how fully this work of the early nineties established certain matters concerning the general method and the technique of science in this field which have been and will be potent guides in research.

The three fundamental principles of method in this field are: to describe a quantity by its position on a scale of measurement, not by a classificatory adjective; to keep in mind the variable error of every determination that one uses; to study things, qualities and events by studying their relations. Every year has reinforced these principles, but I regret to say that by no means all of present investigators of individual and group differences practise them as well as was done by Fullerton and Cattell in '92[3] or by Cattell and Farrand in '96.[4] In the more technical procedures of presenting distributed results and of measuring relatively few individuals, each in many traits, rather than very many individuals each in only one or two traits, Professor Cattell set an example that is still needed. He also led the way in an effort to secure such cooperative standardization of measurements as has recently been the subject of elaborate investigation and report by a committee of the American Psychological Association.

In one respect this early work transmitted to students of individual differences an unfortunate legacy which had been transmitted to it in turn by the standard general psychology of the day. This was the assumption that human nature was organized very largely as a set of formal functions—sensation, perception, imagination, memory, attention, and the like—which, in any one man, operated somewhat uniformly—that is, independently of the particular content on which they operated. The concrete details of the situation which was perceived or attended to or discriminated were supposed to cause little variation in the efficiency of the process of perceiving, attending or discriminating. Certain abilities in a man —such as, to remember a series of digits, or to bisect a line accurately, or to pick out certain letters on a page—were thus dignified with the significance of very general symptoms of the man's efficiency. Furthermore, tests were commonly planned to fit the supposed *form* of operation as tests of 'memory' or tests of 'attention' and the like, in disregard of other important principles of human organization.

---

[3] "On the Perception of Small Differences," Publications of the University of Pennsylvania; Philosophical Series, No. 2.

[4] "Physical and Mental Measurements of the Students of Columbia University," *Psychological Review*, Vol. 3, pp. 618–648.

In the case of the tests of the Columbia Laboratory, however, this dogma was itself subjected to test; and Wissler's computation from them of the amount of resemblance of the condition of one function to other functions in the same individual was of notable influence in showing its falsity.[5]

We are now without any generally accepted hypothesis concerning the organization of human nature to use in guiding studies of symptoms and correlations. Indeed, among the many problems which Professor Cattell's investigations suggest, there is perhaps none that better deserves study by some one of us than this problem of the structure of human nature—the organization of mental functions. It seems rather wasteful to go ahead testing one test after another that strikes some one of us as significant of something. It also seems rather paltry to measure merely special efficiencies in responding to special situations—such as ability to read, ability to sort cards by color, shape and size, ability to add, ability to learn to typewrite, ability to learn to substitute numbers for letters, and the like. Progress is certain in either case, if we calculate correlations and proceed wisely in view of the relations found; but it is likely to be slow.

Professor Cattell himself has suggested the use, as more or less fundamental qualities according to which a man's particular nature may be inventoried, of the following list of traits or series of scales.[6]

These elements of manhood or components in mental structure hale from a mixture of psychological theory and general reflection

---

[5] C. Wissler, The Correlation of Mental and Physical Tests, *Psychological Review, Monograph Supplement*, No. 16.

[6] "Homo Scientificus Americanus" ('93), *Science*, N. S., Vol. 17, pp. 561–570.

Physical health
Mental balance
Intellect
Emotions
Will
Quickness
Intensity
Breadth
Energy
Judgment
Originality
Perseverance
Reasonableness
Clearness
Independence
Cooperativeness
Unselfishness
Kindliness
Cheerfulness
Refinement
Integrity
Courage
Efficiency
Leadership

on human behavior. It is regrettable that the list has not been published more widely and used in a variety of connections. It seems probable that these significant nouns may in many cases be paralleled by natural units of mental organization—atoms in the human compound.

I venture to suggest also as at least a provisional principle of organization the instincts or original tendencies of man as a species, it being my opinion that some of the terms of the above list refer to rather complex concatenations of traits in man's nature which have only the artificial unity of producing some defined result in human life.

Perhaps we do not know enough about man's unlearned tendencies to use them as the basis for an inventory of an individual's make-up, but I should hope that symptoms of variations in the nature and strength of, say, attentiveness to human behavior, love of approval, annoyance at scorn, mastery, submission, benevolence of the motherly sort, envy, attentiveness to things and their mechanisms, "pleasure at being a cause," and love of mental life for its own sake, could be found with comparative ease, and that variations in these and other instincts would be found to be fundamental in causing the concrete differences found in men. For example, the relative strength of the interest in human beings and their behavior and passions on the one hand and of the interest in things and their mechanisms on the other, varies greatly between the sexes, seems to be prominent as between eminent lawyers and eminent mechanical engineers, and very possibly appears in the salesman *versus* the artisan. The difference in strength is easily observable early in childhood if it is marked and should be easily measurable by objective tests.

In this connection I may also mention a possibility which was hinted at in one of Professor Cattell's early papers and has been more definitely suggested by his recent work on the origin of scientific ability. It is the possibility of a semi-Mendelian analysis of human nature by observing its behavior in heredity. I do not refer to the sort of analysis which creates two allelomorphs by choosing two adjectives, measures a man by applying one or the other adjective, and then soberly computes percentages for the offspring of various crosses! Indeed, I am confident that it is not, as yet, worth while to trace hereditary resemblances except for characters which can be measured on a scale with a probable error of not over one tenth of the mean square deviation of the species as a whole. What is referred to is the probability that if anything is fundamental in human nature it will be inherited, and that by discovering which features of human nature do keep parallel with heredity in considerable disregard of counter-attractions in nurture, we may glean suggestions concerning the primary organization of the individuals whom we wish to inventory.

Even without the guidance of any assured doctrine of the general organization of mental functions the measurement of individuals, the comparison of groups and the calculation of both static and dynamic relations has made, as I said, notable advances. The most notable is the mere extension in amount whereby for each study from '90 to '94 there have been, I suppose, a score of equal average accomplishment from 1910 to 1914. Instead of a few students in courses in psychology or children in schools, we are now measuring trolley-car conductors, paranoiacs, thieves, thirty-seven thousand school children in one month, sewing maids, sufferers from hookworm, patients treated for hookworm, telephone operators, children in out-door classes, and wayward girls. Instead of a few tests of the senses and higher processes, we have books full. In 1900 it was hard to find *bona fide* examples to illustrate even what the correlation of an individual's ability in one trait with his ability in another *meant*. It is now hard to condense the facts found into a fair-sized chapter.

Of the advances in method I will mention three—the improved technique for measuring resemblance, the improved interpretation of symptoms, and the provision of sound treatment for measurements by relative position.

The emphasis put by Professor Cattell upon measuring the reliability of any determination received, in 1904, unexpected additional justification. In determining the central tendency of a group, the unreliability of the measurements of the individuals taken singly does no considerable harm. The variable error for one individual acts upon the central tendency for the group as an offset in the long run to the variable error for some other individual, so that, at the worst, it only increases the *variable* error of the central tendency. The variable error for one individual becomes, however, in part a *constant* error for the determination of the variability of the group around its central tendency, acting in part always to *increase* it. Consequently in a measurement of a correlation where the variabilities of the two groups of measures figure in the denominator, the variable error for an individual measure becomes in part a constant error changing the correlation from whatever it would have been from accurate original measures *toward zero*. The practical importance of this "attenuation" of correlation-coefficients is very great. For example the coefficient obtained for the accuracy of judging 10-centimeter lines with the accuracy of judging 5-centimeter lines is about .3 if only one trial with each is used, but is over .7 if enough trials are used to give accurate determinations.

Spearman, who first called attention to this "attenuation" has improved correlation technique in many other ways. Boas, Pearson, Yule and Woodworth have also shared in this work; and the entire subject is far better understood, as well as far more widely understood, than it was ten years ago.

The improvement in the interpretation of symptoms consists in the acceptance by experts of the principle that any measurement is symptomatic of something beyond itself just in so far as it has been proved by its correlations to be. This makes the inferences from mental tests less exciting and far-reaching than they used to be, but enormously safer. It also makes the most important present work in individual psychology the testing of tests themselves. The improvement in the use of measurements by relative position has been described by Dr. Hollingworth.

The psychology of individual differences is now especially affiliated with education, philanthropy, psychiatry and other varieties of human engineering, even with the law-courts, industry and commerce. In these activities of applied psychology we, his students, have been able to diffuse our leader's teachings so as, I hope, to give him some cause for satisfaction. It is interesting to recall in this connection some of the recommendations and prophecies which he made over twenty years ago concerning the diagnosis of mental diseases, vocational guidance, eugenics, and the use of testimony. I quote from his paper of '93 on "Tests of the Senses and Faculties."[7] "When our knowledge of the normal variation of mental processes has been somewhat further increased, its determination will give useful indications of individual condition, progress, and ability . . . . In many cases tests which could readily be made would indicate disease, especially of the nervous system, long before it becomes apparent to common observation . . . . As our knowledge increases we shall, on the one hand, be able to indicate very early in the life of the child tendencies to insanity and other diseases, to dipsomania and other vices, and to crime, and precautions may be taken which will limit the range of these abnormal tendencies in the individual, and prevent their spreading in the race. On the other hand, valuable qualities may be early discovered and developed. Special aptitudes will not be lost through unsuitable surroundings and uncongenial work, but the child may be led to the course of life in which he will be most happy and most useful. Many tendencies are likely to be eliminated or increased in offspring according to the marriage of the parents. While instinct is probably a better guide to suitable marriage than scientific principles, it cannot be denied that parents and the conventions of society have much to do with the conditions under which choice is made. Somewhat of the backwardness of the dark ages may be due to the fact that many of the best men were celibates. If a code of honor (such as obtains in some aristocracies, and prevents its members from marrying below their rank) could be developed, and directed toward eliminating degenerative tendencies, and developing valuable traits, the race would be greatly strengthened and perfected. . . .

---

[7] *Educational Review*, Vol. V, pp. 257–265.

The accuracy of ordinary observation may be tested by requiring the student to draw the plan of the hall of the schoolhouse, or to give an account of some event, say something that happened in the school the day before. Casual observation will be found very defective, and the student will learn that but little reliance can be placed on stories of extraordinary events, or even on the testimony in a court of justice."[8]

As you know, Professor Cattell's research work in recent years had been divided between the problems of university administration and the individual psychology of the scientific men of America. You are familiar with this later work,[9] which includes collecting facts concerning some four thousand individuals, determining the order of merit of those in each science, and analyzing the facts concerning their nature and nurture. It will long serve as a model for similar studies of the top ranks of literary men, engineers, organizers of industry, social workers, lawyers, statesmen and the like.

[8] *Loc. cit.*, pp. 258–263 passim.

[9] Reported in part in the *American Journal of Psychology*, Vol. 14, pp. 310–328, and in *Science*, N. S., Vol. 24, pp. 658–665, 699–707, and 732–742, and Vol. 32, pp. 633–648, and 672–688.

This investigation is still in progress, and its value augments at an accelerating rate with every new item that is added to the already extensive schedule of data concerning each man of science, his family, and his career. Its possibilities are practically limitless.

Some of you have doubtless often regretted that Professor Cattell has devoted himself within recent years so exclusively to the topic of individual differences, to the relative neglect of investigations concerning the time of mental operations, the psychophysic law, fatigue, memory and association in which fields he has shown such mastery. I cannot join in this regret. From the point of view of public welfare, a community's scientific experts are its most valuable property. The conditions of birth of men fitted to recruit their rank and file are the most important of eugenic conditions, since to add one man of the ability, say, of Simon Newcomb, William James, E. B. Wilson, or Jacques Loeb, to the American stock is of greater advantage than to prevent the birth of a thousand feeble-minded or insane. The conditions whereby such a man is enrolled as a worker in science rather than as a country gentleman or trader are among the most important conditions of the selective distribution of human forces. The world has already suffered too much for enticing its able men into careers of military aggression, ingenious disputations and personal domination and by hiring them to buy from it cheap and sell to it dear.

The conditions whereby men of ability to advance truth and justice are given the early training and the later facilities that they require are the most important educational conditions of our country. If it is worth while to find out what training a future plumber needs to make him a competent plumber and voter, it is ten-thousand fold worth while to find out what training and facilities the future leader in science needs to make him do his best as an investigator and expert adviser of governments.

How much we need just such an investigation as Professor Cattell is now making may be brought home to us by our present ignorance concerning these matters which so closely touch our profession, our country, and the advancement of science. If we had perfect power to breed the next generation from one tenth of the present, we should not know which men and women to select as that tenth. If we had perfect power to choose from the fifteen-year-olds now alive ten thousand to be apprenticed to the study of science, we should not know which ten thousand to select. If we had perfect freedom to regulate their careers, we should not know what studies to prescribe or which institutions to enroll them in. We should hardly know even that it makes any difference what institution or studies we do prescribe. For those who at twenty-five did show worth, we should not know what rewards to give, whether to pay them two thousand dollars a year or twenty thousand, whether to pay them by "time" or by the "piece-work and bonus" plan, whether to give them general prestige or to leave them to the distinctions of their own clan. We do not know whether the general plan of control of the universities, by teaching in which men of science now so largely get what society gives them in return for their services to it, is very, very bad or very, very good, though perhaps Professor Cattell would insist that it certainly is not very, very good. All such questions can be answered only by the continuance in a thorough-going way of the natural history of Homo Scientificus Americanus which he is making.

Some of us should be able in the years to come to cooperate in this research. For example, certain measurements which it is impossible for the originator of the research to make himself might be got by some of us in our several institutions. The Rosanoff-Kent association test, though probably not one of very great diagnostic or prognostic power, might well be given to five hundred of the men of science concerning whose general achievements the investigation already provides so much knowledge. The Ebbinghaus test, the Woodworth-Wells test in responding to a series of small problems, and certain tests for selective thinking give hopes of being, when taken together, fairly symptomatic of the total complex of abilities to manage ideas. It would be, in my opinion, well worth while to test scientific men with these.

Or, to take another sort of cooperation, the student-careers of such men of the list as were students say of Harvard College might well be investigated by Professor Dearborn—their academic grades, choices of electives, activities in the management of student enterprises, and the like, could profitably be recorded.

Again, it is a very serious question how far the choice of scholarship and science as a career is due to the absolute strength of the intellectual instincts—to pronounced zeal for doing things with ideas and having other ideas come as a consequence—and how far, on the other hand, it is due to such a weakness of the instincts of mastery, conflict, display and the like, as makes the man in question avoid the close personal relations of bargaining, employing labor, persuading men for or against their interests, and the like. Perhaps, under our present social system, the law, engineering and big business get too many men who properly belong to science, because the life of the teacher and investigator is too tame, too devoid of human passions, conflicts and controls. Perhaps science gets too many men of mediocre intellect who instinctively crave a refuge from the risks and rows, and even more from the domineering, refusing and wheedling, of social enterprises in general. It is a still more serious question whether men of science and scholarship lose in sincerity, freedom and influence either by absorption in impersonal activity with things and ideas or by submission as hired employees to a board of trust or a public which they timorously appease when they might courageously defy it. We can progress toward answering both questions by studies of the activities, tastes and achievements of scientific men during childhood and youth and by studies of the changes which they undergo in accordance with the possession of private fortunes, service in institutions of various types, or opportunity for self-support by invention, writing, or well-paid expert service. In such studies we may cooperate.

The individual differences which distinguish men of science from the modal man, their differences from other men of equal intellectual gifts, and their differences one from another are an inexhaustible field for research, and Professor Cattell has the confidence of scientific men and personal acquaintance with them to a degree that no other investigator probably could obtain. He was obviously the man to make the study, a study which will lead on the one hand to important improvements in public policy in the conservation, increase and utilization of the nation's intellectual capital, and on the other to new insights into the fundamental general laws of human capacity and learning. Simon Newcomb's achievements as mathematician and astronomer are as properly the subject of psychological investigations as his responses to colors, sounds, or weights, and are as likely to lead to those sweeping general laws of the behavior of the world's objects which are the most striking conquests in the holy war with ignorance and error which we call science.

E. L. THORNDIKE

DUE DATE

JAN 11 1993

Printed in USA

BF 21.A7 no30
3 9358 00040896 0

BF 21.A7 no30
3 9358 00040896 0

THE JOURNAL OF PHILOSOPHY
PSYCHOLOGY AND SCIENTIFIC METHODS

EDITED BY PROFESSOR FREDERICK J. E. WOODBRIDGE

There is no similar journal in the field of scientific philosophy. It is identified with no philosophical tradition, and stands preeminently for the correlation of philosophy with the problems and experience of the present.

The contents of recent numbers include:

An Orientation to the Study of Perception: M. T. McClure.
The Case Method in Ethics and Its Critics: George Clarke Cox.
Value and Potentiality: Harold Chapman Brown.
Perception: Isaac Aaronson.
Miss Calkins on Idealism and Realism: J. E. Turner.
The Thirteenth Annual Meeting of the American Philosophical Association: Harold Chapman Brown.
The Twenty-second Annual Meeting of the American Psychological Association: M. E. Haggerty.
The Case of Professor Mecklin: Report of the Committee of Inquiry.
An Empirical Definition of Value: W. H. Sheldon.
Two Factors which Influence Economical Learning: Edward K. Strong, Jr.
Concepts and Existence: Walter B. Pitkin.
Image and Affection in Behavior: John B. Watson.
Comparative Psychology: A Question of Definitions: Robert M. Yerkes.

Published on alternate Thursdays

Sub-Station 84, New York City

$3 per Annum, 26 Numbers **15 Cents per Copy**

ARCHIVES OF PHILOSOPHY

Editorial communications should be addressed to Professor Frederick J. E. Woodbridge, Columbia University, New York City.

Orders should be sent to Archives of Philosophy, Sub-Station 84, New York City. The numbers are as follows:

1. The Concept of Control: Savilla Alice Elkus. 40 cents.
2. The Will to Believe as a Basis for the Defense of Religious Faith: Ettie Stettheimer. $1
3. The Individual: A Metaphysical Inquiry. William Forbes Cooley. $1.00.

THE SCIENCE PRESS,
Sub-Station 84, New York City.